QUARTERLY JOURNAL

OF

MEDICAL SCIENCE.

FEBRUARY 1, 1861.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Reports in Operative Surgery.* By RICHARD G. H. BUTCHER, ESQ., M. R. I. A., F. R. C. S. I.; Chairman of the Surgical Court of Examiners, and Examiner on Surgery in the Royal College of Surgeons in Ireland; Surgeon to Mercer's Hospital; and Lecturer on Clinical Surgery.

1. SUCCESSFUL EXCISION OF THE ENTIRE UPPER JAW AND MALAR BONE, FOR AN ENORMOUS TUMOUR INVOLVING BOTH, AND FILLING THE PAROTID REGION.

2. SUCCESSFUL EXTIRPATION OF THE EYE, FOR TRUE SCIRRHUS OF THE EYEBALL.

3. SUCCESSFUL EXCISION OF THE ELBOW JOINT; NEARLY FIVE INCHES OF BONE CUT OUT; RECOVERY, WITH PERFECT USE OF THE LIMB.

4. SUCCESSFUL EXCISION OF THE ENTIRE RADIUS FROM ONE ARTICULAR SURFACE TO THE OTHER, WITH NEARLY ALL THE FUNCTIONS AND POWERS OF THE LIMB PRESERVED.

SEVERAL cases force themselves upon me for precedence in the present number of these "Reports;" and yet, though having a very short time ago written a memoir in reference to excision

of the upper jaw, and again, more recently, dwelt upon various additional modes of removing the bone and portions of it, I again, nevertheless, select the subject for a first place, because it enables me to pay a passing tribute of homage and praise to the great surgeon who originated this bold project, and who has been recently taken from amongst us, happily according to the course of time; not ruthlessly torn away, but gently removed in old age, while yet in full and vigorous possession of his faculties and mental acumen, covered with distinction and honour. The finger of science, throughout time, must ever point to the name of Lizars, as conspicuous amongst the pre-eminent of the Scottish school.

Case I.—*Successful extirpation of the entire upper jaw and malar bone, for an enormous tumour springing from the former, extensively implicating both, and filling up the parotid region. Mode of securing the patient. Advantages of the free application of the actual cautery. Mode of dealing with salivary fistula.*

Thomas D., aged 45 years, admitted to Mercer's Hospital March 9th, 1860. At this date he was recommended to me by a very able surgeon in the country, as a suitable case for operation. The man's appearance certainly presented very frightful deformity, owing to an enormous tumour, involving the left superior maxillary and malar bones, and filling up the parotid region, on the same side, implicating, far and wide, the important parts traversing the space. The suggested operative procedure of total removal was truly a formidable undertaking, a bold and terrible measure; but, as presently will be seen, practicable as suggested by my learned friend, and happily carried into effect. I shall first briefly detail the history of the case, its commencement and progress—the special operation, its intricate and complicated steps—the pathological states and conditions of the morbid product—and, finally, make a few practical observations, bearing upon the facts. Nearly two years before his admission, he was first apprised of the commencement of those changes which so quickly followed in succession—a dull, heavy pain, fixed in the upper jaw, under the floor of the orbit, and to the outer side of the canine depression; this soon changed its wearying character for one of a more acute and violent nature, coming on at quick and repeated intervals, and usually located in the site of the two last molar teeth. No arguments could prevent the man having the teeth drawn. They were removed, but with no mitigation, or even temporary rest, from suffering. "For six long

and weary months," as he expressed himself, did this deep and boring pain fix itself within the bone—often preventing sleep altogether, frequently impairing it for nights in succession. Yet all this time there was no swelling, no enlargement. However, quickly after this, in the seventh month, changes were being brought about, alterations manifest, and, in quick succession, they supervened upon each other; the hollow beneath the orbit began to disappear; quickly its depressed surface became on a level with the edge of the orbit and the alveolar range; soon again the bone was forced out, as prominently as the malar; and now the sufferings began to assume even a more aggravated type; pain diffused over a more extensive surface; located in organs—the eye, the ear; developed in contiguous surfaces, the nose, the palate, the throat. Thus the case gradually progressed, up to the time of his coming under my observation. His condition altogether was so remarkable, that I shall endeavour to map out all interesting particulars with as faithful and accurate a hand as so serious an investigation merits, nay, even demands. For several months after this time the pain became comparatively trifling, as contrasted with what it had been in the early development of the tumor, even though its magnitude was steadily progressive. Suddenly, however, as the tumor encroached upon the parotid space, his sufferings were again augmented. The appearance of the man, previous to operation, is accurately depicted in plate I. figure 1, from a truthful picture drawn by Connolly, and beautifully lithographed by Forster. It will be seen that the whole countenance was hideously distorted. The tumour now was fully as large as the two clenched fists; springing from within the antrum, it forced out its walls in every direction, impinging on every neighbouring cavity. Its outward direction being most palpable at first, but soon passing mesially, it forced in the nasal partition, thrusting gradually aside the turbinated bones; blocking up altogether the left nostril from the anterior to the posterior opening, and slightly bulging the septum nasi to the opposite side; it did not protrude in front, and only to half an inch behind, being in both these aspects firm and dense to the touch, not yielding blood, even on forcible pressure. By its volume, too, the palate-plate was thrust down upon the tongue three-quarters of an inch below its normal position, and here also the same solid characters marked the growth. There was nothing peculiar in the colour of the part, save that it was of a darker hue, and probably a little more vascular than the same region on the healthy side. This deeply depressed state of the palate did

not compensate sufficiently for the rapidity of the growth and the expansive force exerted by its progress; for the orbital plate of the maxilla was bulged upwards, partly dislocating the eye from its normal bed, altogether from its proper axis, at the same time everting the organ, and making so tense the optic nerve, that the vision of the eye was exceedingly imperfect. On examining with the finger, this change in the bony plate could be felt. We next come to consider the tumour in its anterior and external aspects, involving, as it did, the whole side of the face, from the nasal process to the ear, and particularly from the junction of the malar and frontal bones to the angle of the jaw. The measurements here passed over an enormous projecting mass, so that the integuments were borrowed from neighbouring parts—the nose dragged from right to left, and the mouth in the same direction; the lips slightly apart, and the jaw depressed; being kept so by unnatural encroachment of the growth within acting as a mechanical obstacle, and to a somewhat greater extent in acquiesence to the will of the patient, to admit a free supply of air, in compensation for the obstructed nostrils. Following the tumour outwards, it was found to have incorporated with it the malar bone, throughout its entire extent; and, on opening the mouth, the tumour was found to pass backwards and outwards, blocking up the entire pterygo-maxillary fossa, and projecting beneath and behind the angle of the jaw; while a more superficial part passed externally to the jaw, matting in its surface the structure of the masseter muscle, and becoming imbedded in the parotid. The deep and intricate relations of the growth in this region were alarming. Yet, withal, there was one hopeful feature even in this direction—the same solid character stamped the tumour; in other words, it could, by careful manipulation, be definitely followed by insinuating cautiously the tips of the fingers; at least *thus* I was satisfied in my own mind. The entire expression of the man was marked with grief, and very pitiable. The integument covering the tumour presented throughout nearly a natural appearance; over the most prominent parts of the growth, however, it was of a dusky-red colour, evidently from congestion, owing to pressure from behind retarding its circulation, and probably acting on a larger supply of blood than had been destined for its supply; it was likewise thinned and shining, yet movable upon the tumour; not identified or incorporated with it at any point. The patient permitted the tumour to be handled with impunity; and no pain was elicited during the manipulation, or lurked in it after. To the touch, it was braced, firm, and unyielding

throughout; perfectly immovable from base to apex; and, in all its characters, affording a fine example of osteo-sarcoma.

The man, though afflicted with this formidable tumour, seemed otherwise in good health; and as he pressed strongly that it might be removed, I saw no valid reason against complying with his request. On the 14th of March, 1860, I operated after the following manner. Before, however, entering upon the several steps of this difficult measure, I shall digress for a few moments, to dwell upon the way in which the patient was secured—precautionary measures of the kind not being sufficiently attended to. The man being seated in a strong, heavy arm-chair, made of oak, his feet were raised a few inches from the ground, and lashed to the corresponding legs of the chair, while his arms were secured to it laterally and behind. To prevent any upward movement, a strong slip of wood, about four inches wide, was passed under the arms of the chair, and across the thighs. I would wish to insist very strongly on these directions; because, speaking practically, I have seen before now, the patient, when not properly secured, break away from all restraint made by hands, and only by force dragged back to the position which he should never have been permitted to quit. It seems a cruel act to lookers-on, the preparatory measure of tying the patient; but it is a practice that never should be dispensed with. No surgeon can estimate beforehand the power which his patient may possess of enduring pain; and no patient can be so satisfactorily cognizant of the fortitude he may command, as to resist a goading agony, that, in its intensity, he never could by possibility have even imagined; and, when felt, the all-powerful impulse to burst away and escape is that alone which takes possession of the sufferer. But even the remote likelihood of such a casualty, granted the term to those sceptical, should never be risked: the patient should be so protected as to be incapable of injuring himself. This precautionary security I hold to be the duty of the surgeon to enforce—nay more, it is an extension of leniency to the individual that is to suffer; for by its adoption, rapidity of procedure is facilitated, if intricate dissections are to be executed, the patient's safety is insured; all suffering is abridged; and, when chloroform is inadmissible, the shock of operation is lessened in its duration. A few words as to the support for the head of the patient. I greatly prefer it resting upon the breast of an assistant, to being sustained against a high-backed chair; because facilitating movements in various directions, and for several purposes, viz., the more ready application of instruments, often made to act more effectively by

ever so trifling a change in the position of parts; the facility afforded where cautious and minute procedure is called for, the patient not being able to dip down and droop the head; and, finally, as being more under command to clear out coagula, to facilitate the application of cauteries, the ligaturing of vessels, and the arrest of hemorrhage. It may appear superfluous to enter into these minute particulars. To those accustomed to deal with such severe measures as I am about to detail, they will indorse these precautionary measures with their sanction. As to the man who reads these pages, wrapped in self-sufficiency and egotism, I have nothing to say. But I would speak faithfully, so as to guide the young surgeon; and I think I would be culpable if I did not append these cautions, because, if followed out, acted up to, they will, as I conceive, be powerful accessories towards establishing a confidence and a self-reliance in the operator; and materially tend towards securing a favourable issue, when everything otherwise would seem hopelessly beyond the pale of operative surgery.

The patient thus placed, with a strong light upon his face, I passed a sheathed bistoury into the mouth, its blade being directed from the left commissure towards the malar bone, and its point steadily fixed against the cheek, as high as the connexion between it and the tumour would permit of. The sheath was then withdrawn, transfixion accomplished, and the mouth laid open from within, outwards, along the track of the instrument. A quick dash of blood might have been expected from the facial artery to follow this stroke of the instrument; but the precautionary step of compressing the vessel while bending round the bone was effectively executed in preventing such a loss, until its cardiac end was ligatured. This incision did not at all reach high enough, so the bistoury was laid aside, and the incision prolonged with a scalpel, over the tumour, up to the junction of the frontal and malar bones. The knife was then carried backwards and outwards along the zygomatic arch as far as its tubercle, or junction of the two roots. The flaps thus marked out were adherent and deeply implicated with the prominent development beneath. I next proceeded to dissect up the superior flap from the tumour, commencing in front, and detaching with it the left ala of the nose from its bony connexions. With cautious sweeps of the knife, the parts exterior to this likewise were freed, the blade of the instrument being kept close to the bone; and, while it passed outwards, the infra-orbital nerve was cut, where it emerged upon the cheek; and thus, too, the vessels passing through the same canal. As the knife travelled outwards, great caution was requisite to dissect the flap

off the tumour, without breaking its integrity, the integuments were so strained and thinned from the inordinate pressure exerted by the morbid growth proceeding from within. However, this being satisfactorily accomplished, due care was necessary in lifting the entire flap by cautious touches of the knife, still upwards, so as clearly to reveal the lower segment of the osseous margin of the orbit, and even still further the forced-up plate, constituting the floor of that cavity. This dissection was delicate and complicated, because threatening the distorted organ of vision. The lower flap was next attacked, commencing at the angle corresponding to the outer wall of the orbit, and dissected downwards and backwards. The separation of this covering from the tumour was difficult in front, for the same reason as assigned when dealing with the upper flap; and as the dissection was carried back, the whole parotid region was exposed, being filled up by the excessive growth. The lower flap had to be set free, thrown down considerably below the angle of the jaw, before its superficial outline could be revealed in its entire extent. The man now got copious draughts of wine; for though no blood was lost, yet the shock manifestly produced a very decided effect, though these proceedings were executed with all the rapidity with which the knife could be made safely to travel.

Now that the entire mass of disease was fairly exposed, it was most pleasing to dwell upon the accuracy of the diagnosis, as to the amount of osseous parts implicated, and which must be taken away. It had been surmised, but was now revealed by demonstration, that the entire left maxillary bone, together with the malar, with their several and wide-spread processes, were all hopelessly seized upon, made parcel of, and distorted in its increase.

When the flaps were drawn apart, the superior thrown upon the forehead, the inferior hanging pendulous over the left side of the neck, with the projecting massive tumour between, an appearance was presented sufficiently alarming to cause several of the students to grow sick and faint.

The next step in the operation was the division of the hard parts. By doing so at this stage of the dissection, I was confident I could more easily get at the external and parotidean encroachments of the tumour. To accomplish this end, I drew the incisor tooth on the left side, and then passed one blade of a long narrow bone forceps into the corresponding nostril back into the pharynx, the other into the mouth. In a second the section was completed. The nasal process of the maxilla was next clipped across at its junction with the frontal

bone. The forceps was next applied to the orbital margin, at the junction of the malar and frontal bones on the outside, and the division made into the spheno-maxillary fissure. The zygomatic process of the temporal bone was then cut across, just in front of its tubercle; the forceps was then laid aside. Spreading the end of a towel upon the tumour, so as to prevent the hand slipping, I seized the maxilla, together with the growth, getting my thumbs well in upon the dense orbital margin, and broke down the entire from its posterior attachments. The detachment of the nasal, orbital, and palate plates, was all that could be desired; and then, by a few semicircular wrenches, and breaking down of parts, by the index finger passed far back, all connexions were safely set free, even into the pharynx; and the wrenching of the parts lacerated the vessels so, that no bleeding worth making mention of followed the proceeding. This was all very well, as regarded the anterior and mesial attachments of the tumour; but its most dangerous relations were deep in the parotid space. All the superficies of the growth, as has been already mentioned, was fully exposed; but it required a long, painstaking, and careful dissection, to set it free from the important parts lodged in this region. Behind, the growth covered the external carotid, which was pressed somewhat backwards, and deeper than its normal course, while to its internal side lay the carotid for the brain, in its upward course; in connexion with their primitive trunks passed their corresponding veins, while, surrounding it on all sides, were vessels of magnitude, and nerves essential to life. The division of the several fixed or bony points rendered the tumour somewhat movable, and the crushing of its holdings behind, as before mentioned, increased this mobility,—circumstances of the greatest moment, as facilitating the tedious and dangerous dissection throughout this difficult proceeding. The knife's edge was kept well to the tumour; and wherever it was hard, or firm and smooth, the handle of the scalpel—the end of the index finger better than all—pressed away slightly adherent parts from it. Thus, by cautious manœuvres, by occasional touches of the knife, by breaking down adhesions, by twisting and wrenching with a guarded force the growth from its bed, the entire was brought safely away. But little blood flowed during the outer and posterior dissection; a couple of large vessels had to be ligatured, but not one on the inside. On carefully examining the extensive chasm, not a particle of the tumour could be detected as being left behind; and a similar inference was arrived at from an examination of the specimen removed. I next cau-

terized with the hot iron the entire raw surface within. And now I would wish to say a few words in reference to the use of the cautery. My impression is, that it is not used as much as it should be in operations about the mouth. In all instances, whether there be hemorrhage or not, whether the growth be suspicious as to its characters of malignancy or not, I invariably use it, and that freely. If there be hemorhage, its application arrests the flow; if parts are suspiciously malignant, it destroys contaminating radicals,—tendency to recurrence; if no bleeding, or malignancy, its touch quickly arouses an inflammatory action, healthy in its character, vital in repair. I have no doubt, by this rapid, healthy change, destructive, diffuse, erysipelatous forms of inflammation are guarded against. During the latter stages of the operation, the patient was supplied with brandy, and demanded it with a liberal hand.

All oozing of blood from the parts having ceased, the flaps were adjusted, and held together by several points of the interrupted and twisted suture. The former answered very well where the integuments were thinned, and at the angle above and throughout the horizontal wound; while the latter was most serviceable and efficient at the angle of the mouth, and again about three-quarters of an inch above this point, where the thickness of the parts was not altered or changed. The sutures were sufficiently numerous to hold every portion of the flaps in juxtaposition, sticking-plaster not being at all applicable, owing to the flaccidity of the detached parts. After the tumour was removed, the eye receded from its forced position, and drooped; and, I need scarcely remark, the lids remained open, from the temporary injury inflicted on their nervous supply. The organ was, however, supported in its natural position, in its proper axis, by compresses of lint passed into the chasm beneath, each having a silk cord attached, brought out of the mouth, and secured upon the forehead. By this mode, too, the cheek was prevented from falling back as much as it otherwise would have done. The patient being now sufficiently restored from shock, he was removed, in the horizontal position, from the operating theatre to his bed. The bed was made comfortable for his reception, being heated by jars filled with boiling water; the increased temperature was most grateful to his cold feet and chilled surface. He lay upon the affected side, with the head slightly raised, which position allowed the increased saliva—the weeping of fluids from the cut surfaces—to flow readily from the mouth, without irritating the parts concerned in respiration or deglutition.

The exterior characters of the tumour having been de-

scribed, I shall say a few words as to its internal arrangements upon section.

The results of the morbid inflammatory action were everywhere evident upon the walls and within the maxilla—thickening, condensation of new osseous matter, in all its several changes, from earliest deposition to the completion of perfect bone, thickening of periosteum, enlarged blood-vessels. In other points, bony structure—even of original formation—thinned, and flexible, and elastic, protruded by sarcomatous depositions from behind, characterized the growth; in some places, masses of broken down gelatiniform matter abounded; while in other localities, the deposit was cartilaginous, springy, and tough, like india rubber. In a few positions, the cancelli of the bone, particularly in the body of the malar, were as it were distended, forming numerous chambers, filled with morbid matter; while, pervading its external parts, numerous osseous spherules were developed, some adherent and protruding out from the solid bone wall; while in other localities, detached pieces could be found in cartilaginous beds. Softened patches might likewise be observed, giving much the resemblance of encephaloid cancer; yet, on being subjected to microscopic examination, no such degeneration could be verified as yet to have taken place. I not only examined the growth carefully myself, under the microscope, with regard to the all-important question of malignancy, but I submitted several portions of it to Dr. Mason, the able lecturer on Physiology. His report confirmed the conclusions I arrived at, namely, its non-malignant nature.

In two hours after the patient was put to bed, there came on some slight weeping of blood; but it ceased shortly, of its own accord, being so trifling as not to call for interference. It was, as I have elsewhere mentioned, occasioned by *the first injection of divided vessels by the exalted circulation of reaction.* In six hours after the operation, the cheek was warm, heat well generated over the body, and the man sleeping quietly.

On the following morning, 15th, the report was very favourable: he had a quiet night, slept, took freely of cold chicken-broth, and some wine. The cheek remains well sustained; the eye steadily supported in position, though the lids are still wide apart, motionless, and the conjunctiva a shade vascular; at the same time, the eye dull, its brilliancy lessened.

16th.—Had an excellent night, very little suffering; partakes freely of milk, eggs, beef-tea, and wine.

17th.—Slept quietly, and has partially regained the power of closing the lids; some swelling over the parotid region;

support as before, and a succession of warm stupe-cloths to the inflamed part.

On the 19th I removed the superficial plugs, leaving in the two immediately beneath the eye. Stupes beneficial in depressing the swelling, noted on yesterday; to be continued.

20th.—Removed the needles, and some of the stitches; union of the cheek perfect throughout, particularly in the thicker parts. Suffered the threads to remain where the covering of the tumour was thinned, as the bond of union, though accurate, was yet weak. Nutriment and support in abundance.

On the 22nd, cut out the remaining stitches; union perfect from one end to the other of the incisions. Removed also the remaining plugs; they came away with facility, being coated with purulent matter; and they fulfilled their office well, as the eye retained its natural position, being evidently held up by recent attachments, brought about, completed while the organ was steadied by this artificial support. At this time, too, full control was obtained over the lids; and the voluntary movements were effective in restoring brilliancy and tone to the cornea and its covering.

On the 7th April, I opened a small abscess in the parotid region, or rather in front, over the masseter muscle. From this flowed out pus and saliva in abundance, the latter fluid continuing for some days to drain freely from it. I passed an armed probe with a few threads of silk from the external opening, through the abscess, forwards into the mouth, and tied the seton up; and in a few days, having established a channel for the saliva into the mouth, I pared the edges of the external wound, brought them together, maintained them so by two points of twisted suture, using very fine needles, and in three days the wound was healed and closed permanently, and the saliva continued to pass by the artificial canal into the mouth.

On the 10th, the man was up, and walking about the ward. The cheek healed accurately throughout, somewhat flattened, being slightly corrugated about its centre; but the eye remained admirably supported, and following its fellow in all its motions; and the lids retained their place, and performed their functions as if the parts had never been meddled with. (See Plate I., Fig. 2.) On looking within, the surfaces are all covered with healthy granulations, and almost finally repaired. The functions of deglutition are well performed, and every day he is obtaining the power more and more of eating solids, and swallowing the bolus without hesitation; while fluids pass backwards without the slightest regurgitation through the

nostrils, His speech, too, is becoming again distinct, and he finds no difficulty in expressing his wishes.

A few days later and the man was up, and walking about the ward; and at the end of six weeks from the date of operation, he was sent home to the country perfectly cured.

I shall not leave this subject without directing the reader's attention, which I do with great pleasure, to an able paper on "Removal of the Upper Jaw," by Dr. Z. Johnson, Surgeon to the Kilkenny County Infirmary, and published in this Journal[a], in which he gives the full details of a most interesting case operated upon by himself; and I must say the whole proceeding redounds greatly to his credit, judgment, and skill.

There are several interesting points in the case which I have detailed, for the practical surgeon to contemplate and dwell upon. It teaches the absolute necessity for accurate diagnosis, so that tumours be not meddled with, implicating the base of the skull, and perhaps incorporated with the very membranes of the brain, as I have seen them. It teaches how the surgeon must adapt the line of his cutaneous incisions according to the direction of the bulk of the tumour. It gives a confidence, how that the eye, though distorted, thrust out, and interfered with in its functions, may again be restored, maintained in position, and bring back enjoyment by its recovered powers. Finally, by the successful result, confidence is gained, and operative surgery maintains its exalted position.

Case II.—*True scirrhus of the Eye-ball, occurring as a primary affection, successfully extirpated; with special observations, particularly in reference to the operation.*

True scirrhus of the eye-ball is so exceedingly rare, as a primary affection, that I am gratified at having an opportunity of inserting this very remarkable case in these Reports in Operative Surgery. So seldom has it been met with, that the existence of the affection was doubted by Mr. Wardrop. In his most elaborate work on "Fungous Hæmatodes," he absolutely denies the existence of this affection. At page 87, he says,—"I much suspect whether *cancer* ever affects the globe of the eye in its *primary* form; at least, I have never met with an example of this kind." And again, a little further on,—"I have never been able to obtain an accurate account of a single case, where any of the coats or contents of the eye-ball were the primary seat of cancer." So, likewise, we have no

[a] Dublin Quarterly Journal of Medical Science, August, 1858.

mention made of the affection in Mr. Tyrrell's book. Mr. Lawrence, in a Clinical Lecture, published in the "Medical Gazette," July 2, 1847, gives a brief description of a case, and refers to the specimen in attestation of the subject. I shall briefly give the details in reference to it, and then dwell upon the remarkable facts in relation with my own case. The specimen was removed from a middle-aged man by Mr. Wormald, and deposited in the Museum of Bartholemew's Hospital. It is thus described in the Catalogue of the Museum (Series ix., No. 17):—"The tissues of the anterior and inferior third of the right eye are occupied by an irregular growth of firm and very vascular substance, with a granulated, warty, and very vascular surface. The optic nerve, of which a portion is preserved, is sound. There was no return of disease in the orbit, but the patient died with medullary tumours in the heart and in some other parts, two years after the extirpation of the eye." The condition of the heart is thus described in the Catalogue: —"Part of a heart, in which there is a large mass of firm medullary matter in the substance of the apex of the right ventricle and of the septum. The morbid mass has not altered the external form of the heart; but it projects with a coarsely granular surface into the cavity of the ventricle, and has raised up the tricuspid valve; in its middle, its substance is softened and broken down. The pericardium is in every part closely adherent. The aorta is dilated, and both it and its valves have earthy deposits in them"[a]. The opinion of so eminent and so distinguished a surgeon as Mr. Lawrence upon this specimen should not be omitted, as we would submissively bow to his judgment in many cases; so I shall declare his opinion here. Referring to case No. iv., in his paper, and that which I have transcribed, he writes:—"Cancer of the eye is a rare affection; very few instances have come within my observation. The preparation now before you shows that the disease is less disposed to extend to the surrounding parts than when it occurs in many other situations: the female breast, for example. The anterior segment of the globe is converted into a dense scirrhous mass, of considerable thickness; while the posterior surface and the optic nerve are unaffected. The palpebræ, with their mucous membrane, the portions of muscles of the globe and optic nerve left in the orbit, were removed after death, and are seen in this preparation, free from disease. Thus, there was no return of disease in the part; but the patient died of secondary cancerous affections, having survived

[a] *Op. cit.*, Series xii., No. 60.

the operation two years. It is worthy of remark that, although the primary affection was *genuine* scirrhus, the deposit in the heart had more of the medullary character."

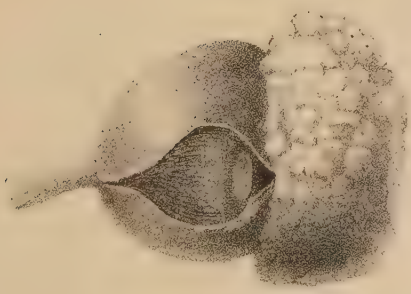
And now with regard to the case which was operated on, in Mercer's Hospital, by myself.

Eliza Doran, aged 69, admitted to Mercer's Hospital, November 1, 1859. Two years before the above date, she was suddenly seized with racking pain in the eye, which awakened her from sleep. On the following morning, the entire organ seemed red, with a profusion of tears flowing from it, which she ascribed to the severe rubbing and pressure that she made upon it to try and get relief from the burning pain in the ball. This pain remained unabated for days, and gradually the sight became dim; she could, for nearly three months, discern objects, but could not clearly distinguish their characters; gradually even this power of imperfect vision was lost; and in five months after the first occurrence of the pain, "a red fleshy pimple" came upon the cornea, and gradually, slowly, increased up to the time of her admission. Her condition, when submitted to my care, was as follows (see Plate II., Fig. 1):—the tumour was fully as large as the section of a walnut, but considerably more prominent; it projected between the lids, and their conformation imparted to it the oblong shape in the transverse axis. However, on separating the lids, the tumour then presented a more circular shape, and was about the size of a two-shilling piece; the growth at its base was considerably less, and seemed to exceed but in a very little way the imbedding of the cornea in the sclerotic coat; from this comparatively narrow base, then, it sprung, and spread out, overlapping to some extent the sclerotic, and projecting between the lids; the entire surface of the growth was mammilated, and made up of firm, dense, fleshy-coloured structure, which, to the touch, was hard as cartilage, did not bleed on being handled—in other words, of strictly scirrhous nature, with a thin watery secretion pouring from it and the conjunctival membrane on either side in great abundance. Pressure with the finger did not pain or hurt; the growth could be handled with impunity, though the wearying lancinating pain seldom ceased; and the exasperated hemicrania, aggravated during night, seldom during these hours was remittent.

Accustomed to meet and deal with such matters, no doubt existed in my mind as to the nature of the case being true scirrhus, set up in the cornea and its conjunctival membrane, and developing itself to the remarkable size mentioned, without involving surrounding parts; for, absolutely, the marginal

Fig. 1.

Fig. 2.



Mr Butcher on Extirpation of the Eye

Mintern Brothers Lithographers

circumference of the cornea to the sclerotic was free from all apparent implication. This was quite clear, demonstrated on lifting upwards, and gently drawing forwards, the morbid product, the lids being restrained apart; all behind this marginal ring seemed exempt from disease. By passing the fingers gently and deeply into the orbit, a certainty of all the contained parts within this bony recess being uncontaminated was conclusively arrived at; the lachrymal gland, buried in its own recess, was free from enlargement, or centered as the nidus of pain. Those darting pains frequently passing through the brow, or those stings, habitual in the tumour, were never transferred to this special and suspicious locality. By the most careful examination I could not detect any other organ implicated in disease, and the woman readily assented to my proposal of removing the part. On the 11th of November, 1859, I proceeded to operate, after the following manner:—the patient was placed recumbent on the operating table, the head being raised by pillows; she was quickly brought under the influence of chloroform; the left eye being the one affected, the head was slightly turned to the left side, so that the blood, issuing from divided vessels, might have a ready drain outwards, and not obstruct careful inspection throughout the after steps of the operation. The head being carefully steadied, the upper and lower lids held apart by approved retractors, a curved needle, armed with a strong silk thread, was passed through the eyeball, or rather, anterior to that part from whence the malignant growth seemed to spring, thus traversing its base from without inwards, from its temporal to its nasal side, avoiding injury to the parts behind, so as to admit of clearer investigation afterwards as to the parts from which the growth had its origin and sprung; the ends of the thread being knotted, the needle was cut away, and thus a means secured of drawing the eye in any direction that might be required, so as to facilitate and expedite the steps of the operation. This being effected, the upper and lower lids were held apart by the retractors, and an incision carried outwards for three-quarters of an inch, disuniting the lids at their external commissure; the upper lid being elevated, and made tense to the degree required, and the eye steadily drawn forwards, I passed a fine long-bladed knife in a semicircular sweep from the inner to the outer wall of the orbit, freely dividing the conjunctival membrane, and carefully the levator palpebræ muscle, and the muscles attached to the ball, throughout this trajet; so likewise the knife was swept in the under corresponding range; a gentle touch on the inside liberated the organ, while a more bold and, at the same time,

scooping evolution of the knife, brought down the lachrymal gland entire from its well-marked depression; the blade of the knife being long, and made in each evolution to move according to the conformation and axis of the orbit, it will be readily understood how efficiently all the external parts were divided and set free; blood flowed freely throughout these incisions; but, from the admirable position of the patient, it did not interfere with quickly prosecuting the end aimed at, the perfect extirpation of the organ by section of the attachments of the recti muscles, and division of the optic nerve and its accompanying vessels far back. This was readily achieved by passing the knife along the outer wall of the orbit well back, the eye being gently drawn forwards and inwards, so as to make tense the parts; by a sudden decided stroke from above downwards, the muscles yielded, and the nerve was severed, and so the parts liberated came readily away; blood flowed briskly, but was restrained by passing a few strips of lint into the chasm, so as completely to fill it up, and by then laying down the lids, and placing a compress outside all, retained by the gentle pressure of a few turns of a bandage; the lids were not stitched where separated at the outer commissure, for reasons presently to be noticed; she quickly recovered from the chloroform, and was conveyed to bed; shortly after an opiate was given, and sleep procured. It is unnecessary to give the daily reports; the patient went steadily on to cure.

On the third day after operation, one of the strips of lint was gently withdrawn, and on the fourth the two remaining stripes; no blood flowed after. The orbit was gently syringed out with tepid water once a day, and a little simple ungent smeared upon the lids, where they came in contact. Steadily the purulent discharge diminished, and ultimately ceased altogether. Sedatives were given to procure rest, and the most liberal diet from the time of the operation to her perfect recovery. She left the hospital on the 5th of December. It was most remarkable the great change brought about in this old creature by so successful an operation; her nights were undisturbed; her appetite was restored, and quickly she gained strength, put up flesh, and never ceased to reiterate her prayers and thanks.

Independent of the external characters, so typical of malignancy, and so readily recognised by the practised eye, the most careful microscopic examination of the tumour led to no other evidence, than the true scirrhous nature of the growth. The characters revealed by the microscope were very analogous to the representation annexed to the case, No. 5, one of the many

figured in an elaborate paper which I published in this journal, "On Encephaloid Cancer, and the Cancerous Degeneration of Warty Excrescences, with the operative treatment applicable to each"[a]. To place the question beyond all doubt, I submitted a part of the tumour, for microscopic examination, to Dr. Mason, the accomplished microscopist, and lecturer on physiology, in the "Original School of Medicine," Peter-street; his account perfectly harmonises with the conclusions arrived at by myself, and in every way confirms the accuracy of the investigation. On prosecuting the dissection with the knife, it was very evident that the entire structures, the whole thickness of the cornea, were matrixed in the abnormal production; and here, again, the microscope substantiated this conclusion. On removing the adherent fat and areolar tissue, and elevating, at the same time protecting carefully, the attached insertions of muscles, all the globe posterior to the encroachment of the morbid growth seemed on the outer surface healthy; the bulk or volume of the part was not augmented; and the sclerotic coat, throughout, presented its normal glistening appearance; the optic nerve was healthy in its section, natural in its form, and unaltered at its external conformation. After the foregoing conclusions were arrived at, from the most careful and repeated examination of the recent parts, then the specimen was plunged into proof spirit, so as to prepare it for an adequate and just examination of the parts within. After five weeks elapsed, it was then carefully examined; and the following conditions testified to the effect of the spirit, so as to render very satisfactory the examination: the globe of the eye, posteriorly, presented its full convexity, and the sclerotic was, as it were, stiffened; a section of it was made with a sharp fine knife, from behind the malignant growth, along its upper surface, and backwards, to within a few lines of the entrance of the optic nerve; its structure was neither thickened nor thinned from its normal state, anteriorly or posteriorly; by separating the edges, a strict and beautiful view of the parts within was afforded; the posterior chamber was not encroached upon in any way; the retina lay as a whitish thin film in plicæ, or folds, in the bottom and towards the lower part of the sides of the chamber, and was natural in every respect, as well as the nerve at its entrance expanding into it. On floating the specimen in spirit, and gently agitating it, the plicæ unfolded, the delicate mesh waved through the fluid, unchanged and unaltered, save by condensation with the spirit. The chamber was not altered in its walls, the ciliary processes

[a] Dublin Quarterly Journal, November, 1856.

were healthy, neither was it encroached upon from before; for the lens lay in its natural position, while the iris was resting against it, but not adherent; and the anterior chamber was obliterated—the cornea, as already mentioned, being not only incorporated, but forced backwards, flattened towards this feeble partition. (See Plate II., Fig. 2.)

On the most careful examination, there was no evidence of contamination beyond the limits already assigned; the strictest microscopical investigation was pursued of every adherent or approximating tissue, the muscular, areolar, &c., the lachrymal gland,—but all seemed exempt from contamination, as might have been with certainty prognosticated from the isolated manifestations of this rare affection.

In Mr. Lawrence's remarks, published in the Medical Gazette, as already quoted, the similitude of the rare specimen which he exhibited to the class bears a most remarkable analogy to mine. See how strictly the description harmonises with what I have written. Now, whether the pathological condition of the cornea described was secondary to the involvement of the conjunctival membrane, I know not, as not having seen the case sufficiently early to determine. I am aware that Scarpa lays the seat of the true carcinoma as being the conjunctival membrane[a]; and Travers expresses himself, likewise, as holding the same opinion[b], and to this effect; so far as present evidence extends, if we except the lachrymal gland, this *membrane* is the only texture connected with the eye, ever primarily affected with carcinoma.

Now, I conceive it necessary to write upon a few points relative to the operation:—1st. On all accounts, it is better to place the patient recumbent, and leaned a little towards the affected side, so that chloroform may be administered to the best advantantage, and the safety of the patient; and that the blood may have a ready escape, and not interfere with the after-proceedings of the operation. 2nd. That the lachrymal gland should be taken away, in all instances, when the operation is for cancer, or when an artificial eye is not applicable, so that no undue secretions may be produced, streaming for ever over the cheek, the delicate lachrymal puncta being probably obstructed, spoiled in their functions, by the resenting inflammation set up by injured parts; this objection to its being left being altogether apart from its liability to secondary infection. 3rd. It is of great importance to the humble sufferer that has

[a] Scarpa on Diseases of the Eye, page 526, Second Edition.
[b] Travers' Synopsis of the Diseases of the Eye, page 90.

been compelled to seek safety for life in this shocking operation, that no disgusting traces should remain, by the lids being apart, as so frequently occurs, with a rather prominent red fleshy protrusion from within; or, if not projecting, at least appearing sufficiently repulsive within the space. Again, when all has been scooped out, the lid retracts, and a dark chasm appears below it; or, again, it shrinks deep back, with the under lid, possibly swollen, vascular, and painful, irritated by excitants from without; to the poor the numerous advantages of artificial eyes, even when applicable, are denied from sheer costliness and expense; in many, even the peculiarities of the case forbid their adoption; yet it is an all-important consummation to avert pain and suffering, to remove revolting deformity. Amongst the wealthy, too, sometimes, art purchased by money cannot separate the afflicted sufferers, in their sad lot, from the poor, as when the artificial eye is not applicable; and then each rests upon the same vantage-ground, the hopes and trust for relief from agony.

Now, when the artificial eye cannot be made applicable, be the sufferer rich or poor, it is of the utmost importance that extraneous matters floating about in the atmosphere, and the sharp winds of a changeable climate, should be guarded off from the delicate and sensitive parts within. I therefore suggest for adoption the method of managing the lids pursued in the foregoing case—namely, after separation of them by section at the outer canthus, and after the several steps of the operation to the removal of the organ, not bringing the divided surfaces in contact by adhesive straps or stitches, but permitting the upper lid to slightly overlap the lower, by a little more than the depth of its tarsal cartilage; if, as the wound heals, the drooping should threaten to be too great, then the lid may be slightly elevated, which can be readily effected by gently drawing it outwards, and maintaining it at the required tension, either by strap or stitch. Sometimes the lid, if redundant, will be inclined to fall inwards; this, too, may be prevented by the insertion of a stitch judiciously placed outside, so as to make a little tense the lid, and, at the same time, give it the least possible tendency to eversion of its tarsal edge; when the wound heals, this will not be found too great. By this method I have secured the most perfect immunity from annoyances by extraneous matters; by this mode I have secured the most placid expression of countenance, free, in every respect, from any repulsive or deformed trace.

The foregoing case is, I think, replete with interest, in many particulars, not alone from the extreme rarity of the affec-

tion, true scirrhus, generated, set up in the cornea and its conjunctival membrane, attacking the ball of the eye, an isolated manifestation of malignant disease; but, again, from the pathological conditions revealed by microscopic investigation, by the most careful dissection, bounding its encroachments with the nicest exactness; and, lastly, and above all, the important and consolatory evidence adduced from well-applied and timely interference.

When particular modes of treatment are brought into practice, and insisted upon, extolled, and based firmly on good grounds, it becomes the duty of those engaged in the work to sedulously guard the superstructure—at least, to endeavour to point out carefully what ought to be done in perfecting the work, not only by precept, but example; therefore I make no excuse for again recurring to excision of the elbow-joint; and I care not if I should be considered tedious, if the facts arrived at be at all proportionate to the deep value I set upon their just appreciation—even one truth realised towards the advancement of operative surgery.

The following case of excision of the elbow-joint illustrates many points of great interest, and forms an appropriate sequel to the papers which I had before the honour of publishing on the subject in this Journal[a]:—

Case III.—*Successful excision of the elbow-joint and adjacent ends of the bones, five inches in all being removed. Great disorganization of the surrounding tissues, yet healthily restored. Recovery, with the motions of the limb nearly perfect.*

M. Reynolds, aged 20, was admitted to Mercer's Hospital, July 16th, 1859, with incurable disease of the left elbow-joint. Ten months before, he got a slight wrench of the joint, the pain and soreness after which disappeared; however, a fortnight had not passed by, when the same annoyance again recurred; he bore with it patiently, and doctored it at home with stupes and herbs; but, matters grew gradually worse, and at the end of the second month he lost all power in the forearm; he could neither flex nor extend it, though retained in a state of partial flexion, with the hand pronated; the swelling gradually increased, the pain became more intense; he had several opinions, and various modes of treatment put in prac-

[a] On Excision of the Elbow and Wrist-Joints, and the Preservative Surgery of the Hand, Dublin Quarterly Journal, 1855; and Reports in Operative Surgery, Dublin Quarterly Journal, February, 1859.

tice for its restoration, but all to no effect; for a time the intolerable suffering would be alleviated, and the high fever subdued; but still each accession and repetition of active inflammation added to its bulk, augmenting the proportions of the limb, even above and below the joint. After the fourth month, abscess formed, and broke by an ulcerated patch above and to the inside of the internal condyle. Others, at a later period, formed and burst on the forepart of the limb. Each suppurative crisis, each evacuated purulent depôt, for the time, gave relief—that is, of course, by comparison with the exacerbation of symptoms ushering in its formation, and the disintegrating process, set up, perpetuated, and completed for its escape; though the pressing symptoms were ameliorated by results such as these, yet temporary indeed was the looked for consummation; the persistent discharge, the irritable sinuses, the stinking fœtor, all remained as so many sources of irritation, undermining the health, and robbing the body of all strength and vigour. Thus, until the period of his being placed under my care, matters became daily worse, both locally and constitutionally, so that life was pressed hard, closely threatened by death.

His condition, when placed under my care, was as follows:—The limb was enormously enlarged, its measurement around the joint being 17 inches, while a corresponding measurement on the sound limb was only 6 inches. From this enormous bulk, the swelling gradually tapered above and below. For 4 inches the humerus participated in the bulk, and downwards for at least 4 inches the forearm was involved. The coloration of the part was also remarkable; and on superficial or casual inspection, a great likelihood to malignant disease of the encephaloid form was manifest. The redness partook of a deep hue in many parts, relieved by patches of modena tint, while yellowish patches were interspersed freely. The several openings presented everted edges, with fungoid, spongy granulations. To the touch, there was great elasticity throughout, and pitting on some parts. When the pressure was carried deeper, matter was even forced to well up from one of the many sinuous tracks beneath. The examination in this necessary and determined way caused great suffering; and the man guardedly steadied the forearm by grasping tightly in the prone position the clammy, wasted, extended hand, with the sound one. The discharge from the several sinuses was profuse; through these sinuses, so extensive, so tortuous, and burrowing, curved probes were passed for many inches, previous to coming in contact with diseased bone; yet that the joint

was irrevocably broken up in its integrity, hopelessly destroyed in its adaptation, was sufficiently manifest from the unnatural mobility displayed in the slightest manipulation of the part. Antero-posteriorly, and laterally, the disintegration of tissues seemed equally advanced; while, from the grating within, it was clear that the articular surfaces had suffered in a like way. The evidence was confirmed by the probe, and the diseased bones tracked out with much accuracy, almost throughout their entirety. I have alluded to the hand, already so characteristic, as emaciated, sweaty, and extended: however, the patient had some power in making the muscles of the forearm act upon the fingers, and ever so slightly flex and extend them; but no power could be excited in the wrist—support being taken away, the hand drooped. All above the swelling, the upper-third of the arm and shoulder were greatly emaciated, throwing into far bolder relief the immense augmentation of bulk produced by infiltration of tissues, and new deposits, all around and about the diseased articulation. Many would suppose, after inspection of the limb—aye, and even after the careful examination of it too, that amputation was more applicable to the case than excision. So emaciated and worn had the young man become, from long suffering, that he came up to town, from the country, to be relieved from his disease. It was quite clear that his constitution could not battle on much longer; already the fatal effects of the unrelenting irritation were becoming more and more manifest. His appetite was gone for months; he was perpetually bathed in sweat, his hair falling out, his pulse double its normal frequency; repeated attacks of diarrhœa, and at times loathing, vomiting, and rejection of all food. Here, then, we have the constitutional manifestation of the local disease sapping the very life. I repeat, many would have supposed the case suitable alone for amputation. However, I did not think so, from experience in serious cases of the kind, and observing the preponderating disorganization and changes as exhibited in the soft parts, consequent upon diseased bones. I am of opinion still, as forcibly expressed in former papers bearing upon the question of excision of joints, that much weight need not be attached to this change in the superficial structures shielding-in the joint; and, as I have before written, this precept may be adduced, *Remove the sources of irritation, the dead bones, and in due time the soft parts will recover themselves.*

I decided, then, on excision in this case, having assured myself that the bones were not corrupted beyond a certain extent, and being practically cognizant of facts which could

not but bear encouragement and confidence to the true interpreter of nature's efforts in the power of compensative adaptation for parts dwindled, decayed, or lost; and thus I rested upon the hope of saving the limb. Again, the power, though a limited one, of flexion and extension of the fingers, was in abeyance; the youth of the sufferer buoyed up the hope of conservation; and, I may be pardoned for saying, the successes of dark and cheerless cases made me cling to this doctrine so forcibly inculcated in the best schools, and which I have myself so warmly espoused, and endeavoured to lift up by my feeble voice and pen, at the same time with a determined vigilance and integrity to be just, for the cause of science, and, above all, in its highest appliance, the benefit of humanity. Having decided upon operation, no time was to be lost; and, on the 22nd July, I excised the diseased bones after the following manner:—

The man being placed lying down upon the operating-table, he was steadily and quickly brought under the influence of chloroform. The affected arm was grasped by one assistant above, and thus the main artery of the limb controlled, and the shoulder steadied. The entire member was rolled inwards, so as to expose its posterior surface, and the forearm and hand steadied by a second assistant. Standing, then, on the left side of the patient, with the left hand fixing the affected joint, I plunged a strong-pointed knife into the soft parts several inches above the articulation, immediately over the posterior aspect of the limb, making a vertical incision, full six to seven inches in length, through the entire depth of the disorganized tissues, down to the bones. Blood did not flow quickly from this extensive wound; and so deep was it, that my index-finger was concealed when exploring the changes wrought within. I could feel the roughened bones freely enough now; but it was apparent that the long single incision would by no means be sufficient, or render practicable the further steps of the operation; so that the knife was carried from the vertical incision, outwards, for at least two inches, and corresponding to the line of flexion, or rather a little above the head of the radius; but, owing to the inordinate thickening of the parts, sufficient room was not yet afforded for dealing with the bones. Therefore the transverse incision was continued from the vertical, inwards, for about three inches. When the knife was first laid on, it was thrust down to the bones; but, as it was drawn inwards, a lighter hand directed it, so that the nerve should not be imperilled. The mesial and deep part of the commencement of the wound then exposed the bones, while the internal

and more superficial division permitted the more ready retraction of the flaps. The disorganized attachment of the triceps was then cut through, flexion of the forearm forced, the shreds of lateral ligaments and supporting tissues of the walls of the joints freely revealed. The end of the humerus was robbed of its cartilage, and adjacent cancellated layers, while the internal condyle and articulating trochlea for the ulna were not only deprived of cartilage, but absolutely separated from the shaft and remnant of the external condyle. The olecranon of the ulna was deeply eaten out in its sigmoid cavity, and its posterior extremity removed; its coronoid process separated and loosened. The head of the radius suffered in a similar way; while, leading from the deranged disintegrated surfaces, the shafts were mortified largely, to the removal of periosteum, and destruction of the corresponding superficial surfaces of the bones. With difficulty they were released from the matted and unnatural soft parts in which they were so deeply imbedded; this can scarcely be estimated, except by one accustomed to deal with such matters. As the knife freed the parts, being carried steadily and closely round the bones, much assistance was rendered by the occasional introduction of the thumb, index, and middle fingers, so as to surround the end of the bone attacked, and force away by pressure the soft parts from it. A small piece of linen carried in before the ends of the fingers not only assisted the manipulation by steadying the grip, but also protected them from being wounded by the sharp and detached spiculæ alluded to; each—the humerus, the ulna, and the radius—being separately dealt with, and successively, in this way, as far as the adherent periosteum and maintaining soft parts, marking out and isolating the diseased from the sound bone, as it were; at this junction the saw was applied—that saw which bears my name, and which is now so universally adopted.

The saw, I say, was employed, after the manner which I have so strongly insisted upon in similar cases; the bones being cut from before, backwards. First, section being made of the humerus, three inches and a half being taken away—the bone, when cut, presented the most healthy state,—in a like way the radius and ulna were divided—that is, from before, backwards. Ample and free exposition of their ends having been accomplished, the head and neck of the radius were removed, and the olecranon process, with the coronoid, and an inch of the shaft of the ulna. This section included all that portion of the radius above its tubercle, and the ulna at the same line. It was most pleasing to inspect their cut ends: they

were hard, healthy, natural; while the insertion of the biceps was left undisturbed—a point upon which too much practical importance cannot be placed, as preserving a salutary influence on the after-motions and perfection of the limb. Large masses of disorganized soft parts were then cleft away; but one vessel required a ligature. The limb was then placed in the box which I use for such purposes, and which will be found figured in the February Number of this Journal for 1859. The sides being let down, the limb rested evenly on its inner and ulnar surfaces. The flaps were first lifted lightly towards approximation, and retained in this state by shreds of lint dipped in cold water. The sides of the apparatus were then elevated, and the limb steadied by the protecting pads placed within. The straps were then buckled, and thus all secured.

The patient was then conveyed to bed, having quickly rallied from the effects of the chloroform; and so admirably did it act, that he was not in the least conscious of pain, and had no remembrance of suffering. When he awoke, he could scarcely be brought to believe that an operation had been performed. After being placed in bed, he got a warm draught of wine, and shortly after an opiate, to induce sleep, as he seemed more than naturally excited: evidently a state induced by a double cause, —exalted happiness, by the rescue from apprehended suffering, and the stimulant effects from the protracted inhalation of the anæsthetic. In four hours after the operation, intermediary hemorrhage set in, immediately after an attack of vomiting, so frequent a sequel after the full exhibition of chloroform. The bleeding was slow and steady at first, and by gentle pressure on the humeral artery, high up, was controlled by the resident pupil for a short time. However, whether through inefficiency, or from the blood finding a ready circuitous course on the exaltation of the heart's action, the flow came on more quickly, rapidly, and persistent, so as to create alarm. A tourniquet had then been applied, and I was sent for. It was clear that a considerable amount of blood had been lost; the bed, pads, and sheeting, bore ample marks to this effect. Several large coagula were around the wounded part. I quickly let down the sides of the box, liberated the tourniquet, and cast it off; cleared away the lint from the cut surfaces; turned out all coagula from the deep parts and fissures; let a stream of cold water trickle over all: no vessel of magnitude yielded the supply. Though warm wine was administered, and warm sponges thrust into the deep fissures, yet still the blood welled up rather profusely from several vessels—separately, with but a small stream, and of little consequence—taken

in the aggregate of moment and of great importance, essentially demanding to be checked. Having torn up several long shreds of lint, and steeped them in a saturated solution of perchloride of iron, each was passed carefully into the deepest recesses of the extensively wounded part, and so on a succession of them, until the entire was filled up. Suitable compresses were placed over these, and the limb was bandaged from the fingers upwards, with a gentle, steady support; and as the bandage arrived towards the elbow-joint, it was carried with the same moderate tightness over the compresses, so as amply to supply the place of the fingers which retained them in their position. The arm, in like manner, was lightly rolled from above, downwards; and so, due support offered in this direction, down to where the compresses lay supported by the bandage first applied. The limb adjusted in this way was again steadied in the box, its sides elevated, and the same steady position maintained. The tendency to bleeding was checked, and no appearance of blood issued from the dressings. Wine was now given abundantly, as the man was prostrated to the lowest degree. Full opiates were given every third hour, ice in the mouth to cool the tongue.

6 p.m. No return of the bleeding, but sickness of stomach very troublesome. Ordered two drops of prussic acid to each draught, and iced wine in two-ounce doses every third hour.

23rd. No return of bleeding; and though the stomach was sometimes rebellious, small quantities of fluids and ice were taken with relish. Sleep at intervals. On the whole, he is as well as possibly could be expected. Though a good deal prostrated, yet the pulse is steady, soft, and equable in its beat. The limb is at rest; no pain from it. To continue the wine iced, and draughts of opium; to have some strong chicken-broth, in small quantities, occasionally. Evening report most favourable. Stomach retains all nutriment; and he has slept quietly at short intervals throughout the day.

24th. Has had no returns of the vomiting, slept quietly, and feels refreshed. Took some toast with tea. Owing to the extreme heat of the weather, I was compelled to remove some of the dressings, pads, &c.; this was done with the least possible disturbance or shaking of the limb; the entire was sustained by several hands, so that no drooping at the site of the excision was permitted to take place. The box was then withdrawn, cleansed, and quickly replaced; caution was taken not to disturb the portions of lint thrust deep into the wounds, but all exterior to these were clipped away, and replaced by fresh portions steeped in spirit lotion. After this, I had the

man gently elevated, in the recumbent position, and placed in a fresh bed. Quickly after he exhibited all the benefits of this change, by falling into a quiet, tranquil sleep, that continued for several hours.

26th. Improved, in a most marked way, in all respects. His countenance has lost its haggard, wretched expression. He sleeps, and takes his food regularly and well. On this morning removed all the dressings from within the wound; the suppuration healthily established helped to loosen and cast them off, while a constant stream of tepid water completed their separation without the least pulling or violence, a point to be laid the greatest stress upon, as a repetition of the bleeding, at this stage of the case, might easily compromise the favourable issue. On the 28th, commenced to give gentle support, and draw up the flaps towards each other with adhesive straps and bandages, all the time cautiously guarding against either shaking of the limb or changing it from its flexed position. Every alternate day this treatment was adopted, and so likewise changing the patient's bed linen, &c., &c. Quina mixture, with small quantities of acid, administered to check some tendency to sweating. Opiates now only given at night. Strong broth, eggs, wine, chop, porter, for nutriment. Under this management, the case progressed most favourably, up to August 8th, when his state is reported in these words:—His emaciation is already greatly removed; he sleeps quietly through the night, without disturbance, and sweating has ceased; his appetite has much improved, and he enjoys his food; his whole countenance has undergone a marked change; he looks intelligent, cheerful, and happy, and speaks with a certainty of soon being well; his pulse is full, steady, and in number 70; the bowels and kidneys perform their functions healthily; and altogether, the constitutional manifestation of relief and comfort is an excellent harbinger of recovery. On this date, the condition of the limb evidences a marvellous improvement, gradual and steady up to the present time—the precursor of that favourable state just described, and stamped on the countenance of the man. The diminution of the bulk of the limb is almost wonderful; in fact, it is one of those changes that must be seen to be believed. The engorged, swollen, puffed-out, unnatural-looking soft parts have altogether put on a new aspect; the sinuses are nearly all obliterated, but little matter weeps from them; the skin, as it were, has regained its elasticity, and disgorged, forced out, the discoloured serum from its tissues; all the deep incisions are consolidated and bound together within; no matter wells up on

compression of the parts; the more superficial portions of the extensive wounds are healed by growth of surface into surface, and the new skin is rapidly creeping in from the margins around, and covering the healthy granulating parts; no more matter is secreted than that sufficient for the protection and well-being of the process of cicatrization; no pain is complained of during the dressing of the part; and the same gentleness, steadiness, and support is as gradually afforded as in all the earlier management of the case; the forearm is kept steady, at a right angle with the arm, due care being taken that the adhesive straps and bandages shall insure their relationship, as well as bind the bones closely together, and thus limit the extent of uniting medium; the box sedulously enforces and guards these several requirements.

August 19th. Up, and walking in the garden. Limb doing well.

December 17th. All nearly healed, and full power in bending the fingers and wrist. The power of flexing forearm, of course, not yet regained.

February 1st. He went to the country, with the limb quite healed, and general health restored.

March 2nd. Had a most favourable account from him. All the motions of the arm steadily returning. He is able to grasp and lift weights with it, and make it useful in all its under motions, while, in a limited way, he is regaining the upward movements. I have beside me this moment a letter which I only received a few days ago from him, and in which he describes his condition as that of perfect recovery; all the motions of the limb being conducted with a steadiness and precision but little different from the sound arm; it is not yet so strong as the right one, but sufficiently so for all ordinary purposes. The conducting of this case to so happy an issue, I look upon as a great triumph to Conservative Surgery. Certainly everything seemed dark and gloomy about it, so closely was he run down, when he came under my charge—the preservation of life, even by dismemberment by amputation, being a proposition that might stagger the most sanguine of success.

I shall conclude this paper by detailing a case in which I excised the entire radius from one articular surface to the other.

Case IV.—*Excision of the radius from one articular surface to the other; recovery, with almost perfect motions and functions of the limb preserved.*

Joseph Falkner, aged 17 years, admitted to Mercer's Hospital, January 1, 1859. Three weeks before this date, the boy met with the following accident, produced in a very unusual way:—he was rapidly running down stairs, in the dusk of the evening, his right hand resting upon the bannister as he slid along; suddenly his right heel was checked by one of the stairs, and he would have been precipitated to the bottom, but for the power with which he grasped the bannister; and so he swung violently round, with so much force as to strike his legs against the rails, and his arm, too; he suddenly let go his grip, and fell down, suffering the most severe pain in the wrist. His mother stuped him, &c., and managed him after her own way, the boy at the same time being very unruly, and incapable of being restrained from going to play with his companions; latterly matters assumed a more serious form: the limb became more painful, red, and swollen, and the boy finally could not obtain any sleep. Thus he was brought to hospital, haggard, thinned, worn out from want of rest and inability to take nourishment; the hand was swollen; the forearm red and discoloured, and enlarged by œdema and engorgement, highly sensitive to the slightest touch. On examining the limb, I readily detected the solution of continuity; the epiphysis was wrenched off from the lower end of the shaft of the radius, while the bone was again smashed high up below its tubercle, and the insertion of the biceps; thus the solid bone suffered, while conjointly there was such laceration of soft parts, stretching and tearing of ligaments, that inflammation had run its course, and extensive abscesses passed up the entire length of the forearm, on the posterior surface, and even between the bones. The patient was so wretched, stricken by starvation and loss of sleep, that I confined him to bed, communicated soothing heat, coaxed him by palatable food to eat, compelled sleep to settle on him, by narcotics, for some hours. I then cut freely down upon the matter, and allowed it to escape, slit the tightened fascia so as to relieve constriction and congestion. I next bandaged the limb from above downwards, and from below upwards, leaving the wound free for escape of all secreted and exuded fluids. The fore-arm and hand were then steadied upon a splint, so as to secure against the least disturbance of the disruptured parts: most liberal diet, quinia.

January 8th.—The lower part of the limb is much im-

proved; constitutional disturbance subdued. He sleeps the entire night, and eats well; another abscess forming higher upon the back of the limb, close to the olecranon process; this is on the seat of fracture; this likewise had to be opened, so as to guard against thumbing, pressing, and squeezing the matter towards the lower opening; the entire limb was then carefully bandaged, and steadied upon the splint.

11th. There is a connection established between the upper abscess and the cyst of the lower, so that the discharge of the two nearly all flows off by the most dependent or lowest aperture.

13th. Had to make an opening on the forepart of the limb, about its centre, owing to matter burrowing in this direction; supported the entire by pads, bandages, and splints; wine, Dover's powder, bark, beef-tea, and wine, freely.

19th. Pressure carefully made each alternate day from above down, and from below upwards, with bandages, the entire steadied on the splint. Thus matters went on gradually mending, up to March 10th, when I had freely to lay open the parts over the external condyle, and give exit to matter, after gentle support; wine, quina, &c.

On March 17th, the limb assumed a more serious aspect, marked indications of increased bulk and swelling, great pain, and the supervention of a brisk attack of erysipelas; this again was assuaged, cured by suitable treatment.

On the 23rd, had to open another extensive abscess above the internal condyle.

On the 30th, dismissed to attend as an extern, so diminished was the discharge, in the aggregate, from all quarters; many of the abscesses were entirely healed; and, though I had my misgivings about the condition, the integrity of the bone, yet no sufferings created alarm; on the contrary, his general condition was visibly mended, and his strength materially built up. He was removed by his friends at this time to the country. However, on the 2nd of December he was re-admitted, with profuse discharge from the limb, streaming from several openings, characteristic of the presence of diseased bone. On examination with the probe, I detected the radius at its lower part denuded, rough, and gritty; while, again, on its central part it was stripped of its periosteum, hard, and resonant. Higher up again, close to its tubercle, it seemed deprived of vitality; a good deal of dense thickened material was thrown into the soft parts around, and so filled the inter-osseous space, as to remove altogether the symmetry of the forearm. Again, this compensating material, this lymph effusion, was laid down in far

greater proportion on the posterior aspect of the limb. When he was brought back to hospital, it was stated, that, after being at home for some time, and the limb nearly recovered, he was thrown from a car, and the delicate arm again severely bruised and crushed; and though kept quiet, and the part constantly stuped, the swelling continued to increase, and soon broke, a quantity of matter being discharged by an opening close to the wrist, and in the site of that which had previously existed there, but until then had remained healed from the time of his leaving hospital. A second opening quickly followed in the centre of the bone; and the original one, high up, never healed, and now discharged more freely than ever. On careful examination, it was quite clear that the radius had perished throughout its entire extent, with the exception of its articular surfaces and neck; and I determined on at once excising the bone, to relieve the constitutional symptoms, so seriously prostrating the child, and threatening life.

On the 3rd of December, the boy was brought into the operating theatre, placed lying down, and brought under the influence of chloroform. I selected the posterior surface of the limb as the direction in which the incision should be made for the extraction of the dead bone. A long incision was carried upon the posterior surface of the thickened covering of the two lower thirds of the radius. The edges of the soft parts being drawn aside with copper spatulæ, and also to a slight extent detached or lifted away, a strong scalpel was thrust through the more dense material, and completed throughout this trajet, perfected section down to the diseased bone. With some additional force, this elastic consolidated material was made to recede, so as to allow a more accurate examination. The radius lay in front of this reparative material, it was ascertained as before detected, separated from its epiphysis below, it was rigidly, however, incased all along, and perfectly immovable above. I then made a second incision over the neck and tubercle of the bone, about two inches in extent; and after meeting with some resistance from the same characterized material as already alluded to, I introduced the blades of a long fine bone-forceps, and clipped across the radius below its tubercle. Having done so effectively, I next, with a very strong forceps, grasped the shaft of the bone, and, after some twisting and forcible traction, extracted the entire radius through the extensive wound below. This was most satisfactory, as there remained after, a smooth hollow recess only, corresponding to the posterior three-fourths of the bone. No new material protected it in front; and in the lower part or third of

the arm, the pulsation of the radial artery was quick, distinctly felt from behind. The only protecting barrier seemed to be a slight thickening or condensation of the intervening tissues in the normal relationship between the vessel and the bone. The exterior incision below was not unattended with hemorrhage; on the contrary, it was sharp and rapid—two considerable vessels had to be ligatured. By the shorter incision above promoting easy section of the bone, without subjecting the part to additional violence, or the system to loss of blood, I conceive, a practical fact is arrived at—a good lesson is taught. A few small vessels required to be ligatured, and then the vacant bed left by the extraction of the radius was filled by a few long shreds of lint, soaked in oil, a bandage applied, the limb steadied on a splint, and the boy removed to bed. After a few days, the lint was removed; quickly granulations made their appearance, of a granular, healthy character; soon the hollow was filled up, quickly the edges of the long wound were approximated, drawn together, united; and, in about three weeks after the operation, the limb was healed throughout. It was solid, firm; and, at the end of six weeks, he was able to close the fingers perfectly, and to extend them, to flex the arm and extend it as well as ever, pronation and supination of the forearm being quite deficient. The new material seemed matted to the ulna; but it was a sufficient bond between the lower and upper ends of the bone—a sufficient sustentation for all the required movements, either delicate and refined, or coarse and violent, save two, pronation and supination,—easily compensated for, however, by neighbouring joints.

The whole case, in its management from beginning to end, teaches a valuable lesson: how youth will repair accidents of the gravest nature, by the vitality of its tissues; how that, even with the severest complications, results may be expected that never could be anticipated in later years; how that when, through additional injury, from accidental circumstances, all nature's efforts at repair may be broken up and destroyed, and absolutely a large portion of the solid part of the limb perish by death, yet how that, in youth, this deadened bone, fatal to life, if permitted to remain, may, by a bold and dexterous operation, be removed; and how it may be replaced by a substitute; and how the limb may be restored to nearly the full performance of all its functions,—*by Conservative Surgery*[a].

[a] The casts taken before and after operation in these several cases, as well as the diseased parts and bones cut out, are carefully preserved in my Museum.

Art II.—*On Croup.* By E. Whittle, M. D., M. R. I. A., Senior Surgeon to the South Dispensary, Liverpool.

In the present day there appears to be, amongst medical men, a degree of uncertainty as to the best mode of treatment which can be adopted in croup. I believe this difference of opinion arises, in some measure, from the confounding of different forms of laryngeal affections,—forms which, when accurately distinguished, will be found to require, in some respects, a very different line of treatment.

I purpose, in this paper, suggesting some points of distinction in the various kinds of croup that are commonly met with in practice, and then detailing the mode of treatment which I have found most applicable to each form.

For practical purposes, I would divide croup into seven varieties: strictly speaking, these cannot all be regarded as real croup; but they are all affections of the larynx, attended with constriction of the glottis, and all liable to be confounded together under the name of croup.

1. *The Cynanche Trachealis* of Cullen: true croup, with formation of false membrane.

2. *The Angina Stridula* of Bretonneau: pseudo-croup of Guersant; acute asthma of Millar.

This is a form of croup characterized by spasm of the glottis, with intense inflammation of the lining membrane of the larynx and trachea, but without formation of false membrane. This form of croup has not been generally distinguished in the child from true croup. Bretonneau and Guersant endeavour to distinguish them; but, throughout the whole of their observations on cases of this kind, they create confusion by confounding this form, which is in fact an acute laryngitis, with laryngismus stridulus, which is purely a nervous affection.

3. *Croup, complicating Diphtheria,* the diphtheritic exudation gradually extending into the larynx.

4. *Sympathetic Croup,* connected with the exanthematous diseases.

5. *Spasmodic Croup,* excited by an ulcerated state of the larynx.

6. *Mechanical Croup,* caused by œdema of the glottis.

7. *Nervous Croup,* Laryngismus stridulus.

1. *Cynanche Trachealis.*—This form of croup is characterized by the suddenness of the attack, and by its generally coming on in the night. A child goes to bed apparently quite well, and after two or three hours' sleep awakes suddenly, nearly

suffocated: the symptoms are so urgent, that, when remedies are not promptly applied, death frequently takes place in a few hours. The symptoms of this form of croup are so familiar to us all, that I would not allude to them, except for the purpose of noting the points of difference between this and the second form.

2. *Cynanche* or *Angina Stridula.*—Croup, with spasm of the glottis, excited by the inflamed condition of the mucous membrane of the larynx and trachea, but without formation of false membrane. This is, in Liverpool, the most common, the most dangerous, and the most fatal form of croup; but is, I believe, almost universally, in practice, confounded with cynanche trachealis. It commences with an occasional hoarse cough, which generally excites very little attention on the part of the parents or nurses: this sometimes lasts for two, three, or four days; perhaps a little cough medicine may be given, which affords some temporary relief; but at last the breathing becomes croupy, and then, for the first time, the parents take alarm. When the doctor arrives he will find the child in a high fever, with breathing very quick and hoarse, but not so loud as in true croup; and every now and then the child coughs with a sharp, ringing, barking sound, which is quite characteristic of this form of croup. The peculiar noise of the cough seems to be occasioned by the forced expiration of a volume of air through the contracted glottis, by each act of coughing. If a child in this state is treated in the old routine way, by leeching, emetics, antimony, and calomel, a little temporary relief may be afforded; but, at the end of a few hours, the breathing becomes gradually more stridulous, the child throws back its head, and struggles almost convulsively for breath, throws its arms about, and stretches forth its hands, as if imploring assistance; the pulse becomes very weak and rapid, and copious perspirations stream down the child's neck and breast. If tracheotomy be not now performed, the child very quickly dies, suffocated.

This form of croup is accurately, though only incidentally, described by Bretonneau; but he appears to consider it of little importance. I quote from his second memoir on diphtheria, as republished by the New Sydenham Society: in speaking of the treatment of diphtheria, he says, "a simple tracheitis, or even a very mild sporadic affection, which is, perhaps, the same as that designated by Millar his acute asthma in the first stage, has made the reputation of the most accredited methods. This sporadic affection, which simulates croup, is not at all rare; it commences as it terminates, that is to say, from the

beginning, the alteration of the tone of the voice, the peculiar sound of the cough (resembling the barking of a young dog, heard at a distance), and the spasmodic difficulty of respiration, would cause the most fearful apprehensions, if any fears were not counteracted by the rhythm of the circulation, which is not so much disturbed as it is accustomed to be at this stage of true croup, and if we had not an additional reason for security in the natural state of the pharynx, and in the absence of swelling of the lymphatic glands, which in epidemic croup are constantly tumefied in the regions corresponding to the affected membranes."

Possibly in the comparatively dry climate of central France, this form of croup may not be serious. A sub-acute laryngitis may take place in a child and subside in a few days, without giving rise to alarming symptoms; but I unhesitatingly affirm that, in our cold and damp climate, particularly in the winter months, this affection is the forerunner of the most fatal form of croup: possibly, if life lasted long enough, false membrane might form; but I have always found that death ensues a very few hours after the commencement of the spasmodic breathing, —indeed, I do not think the child ever survives more than from twelve to sixteen hours, though the incipient stage, described by Bretonneau, may have existed for several days, or even for a week.

At page 115 of the same work, we find a chapter devoted to the diagnosis of this form of croup, as distinguished from croup caused by diphtheria; but, throughout the whole chapter, he appears to confound it with laryngismus stridulus (a purely nervous affection). But still, in describing the severest case that he had met with, he concludes with the remark that "the mucous membrane of the larynx was undoubtedly affected with a rather acute catarrhal inflammation." Under the name of "tracheitis," he then describes three cases of this disease, two of them terminating fatally. The post-mortem appearances are thus described:—"The base of the tongue was very red; the mucous membrane of the trachea and bronchi was thickened, and of a uniform deep red colour; the back of the mouth was filled with mucous matters of a grey-greenish tint." The state of the larynx is not specially mentioned.

The second case recovered.

"Case iii.—The opening of the body confirmed the opinion which had been offered, that no false membrane existed in the trachea; the mucous coat of the larynx and trachea was thickened, and of a very bright red. A small quantity of

creamy puriform mucus (about three drachms) filled the first divisions of the bronchi. I did not pay sufficient attention to ascertain whether the dimensions of the glottis were sensibly diminished by the tumefaction of the mucous folds of the larynx. It did not appear possible that the mucus accumulated in the principal divisions of the bronchi would have opposed an insurmountable obstacle to respiration."

I have made two post-mortems in cases analogous to these described by Bretonneau; in one the post-mortem appearances were exactly the same; in the other, besides the redness of the mucous membrane, the glottis was contracted by chronic thickening of the folds of the mucous membrane and submucous interstitial deposit; but in this case the child had been, for more than two years, subject to repeated attacks of croup, which eventually became chronic, an acute attack supervening whenever the child caught cold. It was eventually carried off in a few hours by one of these attacks, the parents refusing to consent to the performance of tracheotomy. Guersant distinguishes this form of croup from the membranous form, but, like Bretonneau, writes as if he did not look upon it as at all a formidable ailment. Jurine appears to have described it as intermittent croup. Of three cases which he describes, one would appear to have been fatal, and the post-mortem appearances seem to have corresponded with those described above.

Bouchut distinctly confounds this disease with laryngismus stridulus. At page 279 of the work of the New Sydenham Society, quoted above, we read:—"The disease which has been most frequently confounded with croup, and which has given rise to mistakes which are fatal in their results, is false croup, or stridulous laryngitis. It is important to establish a precise distinction between these two affections, which require entirely different therapeutical appliances. M. Guersant has contributed more than any one to clear up this subject, which remained so long confused and obscure."

"Stridulous laryngitis is accompanied, like croup, with a dry, hoarse, sibilous, and more or less sonorous cough. The difficulty of breathing is extreme,—the child appears as if about to perish of suffocation; still the larynx is free, and there cannot be any expectoration of false membranes. The phenomena which are observed are purely nervous; they soon subside, and their progress is altogether peculiar. They appear suddenly, and in a very high degree of intensity, in subjects who are otherwise in good health, or slightly affected with cold. They manifest themselves in the middle of the night;

the paroxysm lasts about two hours, and is reproduced on the following two or three nights in succession; but it becomes gradually more feeble, and at last it disappears."

Here are stridulous laryngitis and laryngismus stridulus, two distinct and formidable diseases, completely confounded together, and both treated as trivial complaints; while one is a most dangerous form of sub-acute inflammation, and the other, though simply a lesion of innervation, yet not infrequently fatal in its effects.

To resume, this form of croup is distinguished from cynanche trachealis, by the occasional barking cough existing for two or three days previous to the acute symptoms setting in; the same feature, with the sthenic character of the accompanying fever, and the absence of white patches in the throat, will distinguish it from croup complicated with diphtheria. How it could be confounded by a practical physician with true laryngismus stridulus, I can scarcely conceive,—the latter being a purely nervous affection, generally, if not always, arising from the irritation of dentition, always coming on suddenly with a slight convulsion, followed immediately by a loud, crowing, inspiration; these symptoms sometimes recur again and again, with brief intervals; but, throughout the attack, the breathing is not at all croupy,—the crowing sound of the inspiration being the only resemblance to croup that presents itself.

3. *Croup complicated with Diphtheria.*—This is really diphtheria, gradually extending into the larynx. Before the symptoms of croup present themselves, the condition of the tonsils will have sufficiently declared the true character of the attack.

4. *Symptomatic Croup.*—This is a rare form of croup, occasionally met with in the early stages of the exanthematous diseases: it sets in suddenly, with high fever, and very loud croupy breathing; if the eruption comes out well, the croupy symptoms will, I believe, subside spontaneously. I am not aware that there is any symptom by which we can recognize a case of this kind, until it is declared by the appearance of the characteristic eruption. I have myself met with but one case of this kind. The patient was a strong working lad, fourteen years of age; I found him in a high fever,—skin dry, pulse sharp and quick; breathing very loud and croupy. I bled him freely from the arm: this mitigated, but did not entirely remove, the symptoms. At the end of a few hours the rash of rubeola broke out all over him, on which the croupy symptoms entirely disappeared, I must beg the reader not to con-

found this form of croup, which is very rare, with those cases, pretty often met with, in which different forms of croup arise, in the advanced stages of rubeola, scarlatina, variola, and erysipelas.

5. *Croup caused by an ulcerated Condition of the Larynx, either Syphilitic, or following the Ulcerated Throat of Scarlatina or of Variola.*—I believe that in this form the immediate cause of the croupy breathing is spasm, excited by the irritation of the ulcers.

6. *Mechanical Croup.*—Œdema of the larynx, generally a complication of œdematous erysipelas, or ulcerated larynx, may, after being long chronic, give rise to mechanical croup, by causing a puckered condition of the glottis; in either case, nothing can give relief but tracheotomy.

7. *Nervous Croup: Laryngismus Stridulus.*—This is purely a nervous affection, generally a complication of dentition; it appears to be very much connected with the constitution, and in some children is excited by various causes,—over-feeding, cold, &c., but generally when the child is teething. It comes on suddenly, with a convulsion, generally slight, but sometimes of a more severe character; this convulsion is followed by a shrill gasping inspiration, something like the whoop after a paroxysm of hooping-cough, but never so long nor so loud; it is often cut short by a slight return of the convulsions, and so repeated again and again for several minutes. The attack then passes off; but, in children who are very subject to it, is often repeated once or twice in the same night, with intervals of one or two hours. It is only occasionally that medical men have the opportunity of witnessing these attacks, because, by the time a medical man arrives, the child has quite recovered; but it sometimes happens that the child is suffocated at the very outset of the attack. In these instances death is always sudden; the child may be as well as usual, perhaps sitting on its mother's lap, when it suddenly becomes stiff, struggles for an instant, and dies before the crowing inspiration takes place at all. I believe that many children who are said to die of fits, or to be overlain, die of this affection. I have myself known two children to die exactly as I have described these fatal cases; and these two children were both very subject to laryngismus stridulus. In each case I was sent for immediately, arrived in a few minutes, and yet on each occasion the child was dead: one was nine months, and the other sixteen months old.

With regard to the pathology of this disease, I have always observed that it is most met with in families of a strumous

diathesis; and I believe it is a nervous lesion, excited by different irritating causes in weak strumous constitutions; and this, I am inclined to believe, is the true explanation of Dr. Ley's theory, that it is occasioned by the pressure of enlarged cervical glands on branches of the par vagum. If his theory were the correct one, why should the affection be so irregular in its periods of attack, while the glands continue enlarged? And how comes it that we often see children with masses of enlarged glands in the neck who do not suffer from this affection; while, in very many instances, in which children are very subject to it, there is hardly any evident enlargement of the glands at all?

Treatment.—Of late years I have, in the treatment of croup, widely departed from the old orthodox plan of bleeding, and administering tartar emetic and calomel freely: I do not claim any originality in this, because I am aware that a feeling has been for some time gaining on the profession, that the treatment of croup had long been of too severe and energetic a character; but that very many practititioners still have faith in the old system is, I think, evidenced by the fact, that the New Sydenham Society have re-published Bretonneau's recommendation of what I think may, without impropriety, be termed the immoderate use of calomel, without a single note of objection; neither, in any critical notice of the work, have I seen any exception taken to this treatment. Since I ceased to bleed and give tartar emetic and calomel, I have been much more successful in the treatment of croup. For *cynanche trachealis* I have recourse, in the first instance, to the warm bath and an emetic: this will arrest an attack in the early stage as effectually, if not more effectually than leeching. If the symptoms persist, I administer small doses of compound ipecacuanha powder, with nitrate of potash, every hour, and apply hot fomentations to the throat; this treatment, without lowering the strength of the patient, will do more to abate the croupy breathing than calomel or tartar emetic. If the tongue is foul, or the bowels loaded, I give a dose or two of calomel with the first doses of Dover's powder. If, in spite of treatment, the disease progresses I would perform tracheotomy early, before the patient's strength was too much reduced, and before the false membrane became deposited far down the trachea: I say I would, because, though I have often recommended the operation in such cases, I have never yet been able to get the consent of the friends until it has been too late to perform the operation with the slightest chance of success.

For *stridulous angina*, as I have described it above, I would pursue the same course; but in this form of croup the point is

to attend to it before the urgent symptoms come on, during the premonitory stage, which may last for several days: even then it will take from four to five, or even seven days, before the child can be considered safe; that is, before the shrill, barking cough has completely disappeared. If no attention is paid to the child until his breathing becomes decidedly croupy, medical treatment is of little avail; and, if not affording relief in a very few hours, should be abandoned, and recourse had to tracheotomy: and it is in this form of croup that tracheotomy is pre-eminently successful, because there is no false membrane to stop up the tube, or produce suffocation by lodging below it.

Last January, I saw, one night, a little child, two years old, who had a barking cough for two or three days; his breathing was croupy when I saw him, but the case did not appear very urgent, so that I hoped it was not too late to relieve the symptoms by medical treatment. I saw him early in the morning, and found that the case had then become much worse, in fact, that suffocation was imminent: I recommended tracheotomy to be performed immediately. The parents consented, and the operation was soon performed. Just as I was opening the trachea, the child gave a sudden struggle, the trachea slipped off the hook, the air rushed through the opening, and the wound was immediately filled with bloody froth, and the child fell back, apparently lifeless; I immediately passed a probe into the trachea, raised the child's head, and then, after he was relieved by a convulsive cough, introduced a small forceps, by which means I held the trachea open for a little until the child breathed freely, and then I succeeded in getting in the tube; as soon as it was adjusted, the child was quite at ease; he steadily improved, and in a few days was able to walk about his room. Whenever the tube was withdrawn, he began to breathe rather in a croupy way; on this account, I allowed him to continue the use of the tube for some months. It was not finally removed until May; the orifice closed in a few hours; he has enjoyed perfectly good health ever since.

The treatment of *diphtheritic croup* is rather foreign to the object of this paper, so I will merely observe that I believe, when diphtheria becomes complicated with croup, the best chance for the patient would be an early recourse to tracheotomy. For *sympathetic croup* we must necessarily pursue the same treatment as for cynanche trachealis; from which it can only be distinguished by the appearance of the rash, upon which making its appearance, the croup will spontaneously subside.

Croup caused by ulcerated larynx may be often treated successfully by tracheotomy. I have three times performed this operation after scarlatina. In the first case the child did well for a fortnight, though he was anasarcous at the time of the operation, but at the end of that time he was carried off by an attack of acute bronchitis; in these cond case the operation was unavailing, as the child died the next day; in the third the case went on very favourably for several days, but the child was very badly nursed, and no care taken to keep the tube clear of mucus, consequently the improvement did not continue. All these cases were operated on under very unfavourable circumstances. While this form of croup is chronic, the treatment will principally consist in the application, topically, of solution of nitrate of silver, or such agents, to the larynx itself.

Mechanical croup from œdema of the glottis, if severe, can only be relieved by a timely operation; the hindrance to breathing being purely mechanical, the relief given in these cases is instantaneous, but the operation should not be too long delayed.

With regard to *laryngismus stridulus*, the treatment is so opposite to that indicated in true croup, or stridulous angina, that it will be sufficient merely to allude to it as consisting, at the time of the paroxysm, in dashing cold water on the child's face, or plunging its feet into hot water, or in fact doing anything which will give a sudden shock to the system, and, in the intervals, strengthening the child by the use of tonics, and by having it almost constantly in the open air.

In conclusion, I must again express my surprise that nearly all the French authors, whose writings have been translated by the new Sydenham Society, seem to concur in misunderstanding this nervous or false croup. The passages I have quoted above from Bretonneau, Guersant, and Bouchut, show that they have all confounded stridulous laryngitis with nervous croup, at the same time that they seem to think more lightly than we do in this country of the danger of either complaint. Even in the brief memoir of Daviot, he seems, in the following passage ,to fall into the same error. Writing on the *sonitus crepitans* of Albers, he observes: "Now to apply to pseudo-croup, which requires only the treatment applicable to slight colds, the medication necessary for tracheal diphtherite, would be to employ therapeutical measures which are not only useless, but are even likely in themselves to prolong the duration of the disease."

The term pseudo-croup will be understood in England to

apply to laryngismus stridulus, while his allusion to the treatment applicable to a slight cold shows that, while he wrote, he was thinking of the premonitory stage of stridulous angina.

On referring to Dr. Copland's Medical Dictionary, I observe that the same mistake of confounding these two forms of croup prevails throughout the whole of his elaborate article on croup.

This paper would be hardly complete without notice of yet one other form of croupy breathing, caused by the pressure of tumours, generally aneurisms, on the recurrent nerve. It is difficult to suggest a remedy for this form; the symptoms are generally remittent; but I can imagine cases in which tracheotomy might be justifiable, as a means of affording temporary relief.

P. S.—The following case, being of some interest in respect of the question of practising tracheotomy in croup, I am induced to add it to the above paper as a postscript.

November 6th. I was this evening requested to see a child three and a half years old, which then presented the appearances that I have enumerated as characterizing the early stage of stridulous angina. The child was playing about the house as usual, and now and then coughing in the peculiar shrill barking manner that I have described. I warned the parents of the child's danger, and ordered a warm bath, and a diaphoretic mixture. The next day I found the child worse. It had not had the warm bath, and had even been allowed to go in and out of the house. The breathing was now quite croupy, the barking cough more frequent, and the child labouring under a good deal of fever. This day the child was more carefully attended to, and in the evening there was a decided improvement, though the symptoms of croup were still present, but more mitigated in character. On the morning of the 8th the improvement was less marked; and in the course of the day it became evident that the child was gradually getting into a hopeless condition, as far as the administration of medicine was concerned.

Under these circumstances, and being convinced that no false membrane existed to interfere with the subsequent treatment, I recommended tracheotomy being had recourse to: this was strongly objected to by the friends, but eventually they consented, after I had long urged the necessity of the operation with a good deal of decision. The operation was accordingly performed, about 10 o'clock on the night of the 8th. The child's breathing immediately became natural; she slept

two or three hours that night. In three days she was running about the room; and on the 27th, nineteen days after the operation, I finally removed the canula; the child has continued quite well ever since. A little muco-purulent phlegm was expectorated from time to time, through the canula, but no false membrane whatever—this result of the operation confirming my diagnosis, that this was not a case of cynanche trachealis. Still, so urgent were the symptoms, that, but for the operation, the child must have sunk in a few hours, probably in five or six.

Art. III.—*Pseudo-colloid Ovarian Tumour, weighing* 28 *lbs., in which the operation of Ovariotomy was performed successfully; with Remarks.* By D. Lloyd Roberts, M. D., M. R. C. S. L., and L. S. A.; on the Medical Staff of Saint Mary's Hospital for the Diseases of Women and Children, Manchester; Fellow and Honorary Local Secretary of the Obstetrical, and Corresponding member of the Epidemiological Societies of London, &c.

On August 25, 1860, I saw, for the first time, Rachel B. of Lombard-street, aged 35, married, and having three children.

History.—Menstruation commenced at 14; generally lasting seven days, and has been very copious for the last twelve months. It is also, in general, "difficult" for the first two days of each period. Yellow leucorrhœa has prevailed during the intervals of each monthly crisis, both before and since her marriage. She has been more or less under the care of a medical practitioner during the past twelve months, who supposed her case to be pregnancy. She first noticed "a swelling" in the right side of the abdomen, fourteen months ago. The menstrual flux still continuing, and the abdominal tumour also enlarging, and not feeling satisfied with the prolonged duration of the tumour, as the full period of utero-gestation had passed by, she determined upon seeking other advice, and accordingly I was requested to see her. On my visit, I found the patient a tall, spare woman, of sanguineous temperament, and looking much emaciated, though (she informed me) she had previously been stout and healthy. On making an abdominal examination, I ascertained that that cavity was enormously distended by ascitic fluid; and, on making firm pressure to the right of the umbilicus, a solid resisting tumour could be distinctly felt. In addition to the emaciated condition before mentioned, there were an anxious expression of the coun-

tenance, a small, weak, and frequent pulse, anorexia, the foul tongue of sulphuretted hydrogen dyspepsia, constipated bowels, sleeplessness, dysuria, and frequent desire to void urine: the latter, after standing a short time, presenting a copious deposit of lithates. I prescribed a sedative draught, to be taken at bed-time, and, after explaining to the patient the nature of her case, informed her that I should call upon her next day to tap her. This I accordingly did, and was accompanied by my esteemed friend, Mr. Runcorn, of St. Mary's Hospital, who at once agreed with me as to the nature of the disease. She was then tapped, and 14 pints of thin and highly albuminous ascitic fluid were drawn off. The abdomen being then examined, a large solid tumour was found, occupying each side of the abdominal cavity, on a level with the umbilicus, extending upwards to midway between the umbilicus and the scrobiculus cordis, and downwards, on the right side, into the iliac fossa. It could easily be rolled from side to side; and the patient, when turning herself in bed, instinctively supported the tumour with her hands. A vaginal examination disclosed that the uterus was somewhat prolapsed—a condition arising probably from the pressure of the tumour above; it was also perfectly movable. Elevation of the tumour by pressure with the hands was found to produce but slight alteration in the position of the uterus—thus indicating that the tumour was not connected with that organ; and, on examining with the finger on all sides of the body of the uterus, as far as the walls of the vagina would allow, the uterus and the tumour appeared to be entirely unconnected. A pad was now firmly applied to the abdomen, by means of a broad binder; and half a grain of hydrochlorate of morphia was administered in a sedative draught. We then left. In fifteen days the peritoneal cavity had become again very much distended, and tapping was again resorted to. It was performed this time by Mr. Runcorn, and the quantity of fluid which was drawn off by the canula, together with that which oozed away afterwards, was estimated at 13 or 14 pints. She was now recommended to become an in-patient of St. Mary's Hospital, into which institution she was admitted on September 27. Two days afterwards a consultation on the case was held, and ovariotomy decided upon, as affording the only chance of preserving the life of the patient. She was also seen by my friend Dr. Radford, who concurred in the opinion of the desirability of ovariotomy. I made the result of the consultation known to my patient, and fairly laid before her the formidable nature and danger of the operation. She bore the communication with the fortitude of

a Christian, and consented, "if I thought there was no other chance," to place herself entirely in my hands. The few succeeding weeks were occupied in preparing the patient for the operation; during which time the ascites so much increased, that I deemed another tapping advisable. This was performed on October 19, when 9 pints of fluid were drawn off, and a large quantity (estimated at 10 pints) oozed away in the course of the three following days. The treatment, from her admission to the time of the operation, consisted principally in supporting the system by good diet, and in giving her iron and quinia, diuretics, iodide of potassium, &c.; and the following sedative pill to relieve pain:—Hydrochlorate of morphia, $\frac{1}{3}$ grain; extract of cannabis Indica, 1 grain; extract of hyoscyamus, 2 grains.

On November 1, I performed the operation of ovariotomy, in the presence of my colleagues and a number of professional friends; chloroform having been carefully administered by Mr. Runcorn.

Considerable difficulty was experienced in bringing the patient fully under the influence of the anæsthetic; and some vomiting took place, notwithstanding that (in anticipation of the chloroform) all food, with the exception of one cup of tea and a little dry toast, had been purposely withheld from the patient that morning.

The operation was commenced by making an incision three or four inches long, from one inch below the umbilicus downwards. The peritoneum was in this manner exposed, and then divided on Key's Director: some ascitic fluid now escaped, but not much, as she had been tapped only fourteen days before. The peritoneum covering the tumour was next carefully divided on the director, and a large cyst came into view. This was opened by Mr. Spencer Wells' large trochar, and about 10 pints of thick mucilaginous fluid were rapidly drawn off. It was now perceived that the remaining, and by far the larger portion of the tumour was solid, only a few small cysts being scattered here and there on its surface. I passed my hand carefully round the tumour, and found that there were no adhesions.

My friends, Dr. Radford, Dr. Stephens, and Mr. Lynch, the two latter of whom were kindly officiating as my assistants, here suggested that the incision should be enlarged, as it seemed altogether insufficient for the passage of the tumour; accordingly, it was extended above and below, to the length of nine inches. Through this opening I drew the tumour,

which was then firmly held by Dr. Stephens and Mr. Lynch. The pedicle, about four inches long, and an inch and a half broad, was tied by being transfixed twice by strong ligatures, and was then divided close to the tumour. The surface of the peritoneal cavity was now carefully cleared, by means of a sponge, of some fluid and clotted blood which adhered to it; the left ovary and the uterus were next examined, and found healthy; and the wound was carefully closed by means of four silver hare-lip pins, passed through the divided edges of the abdominal parietes, including the peritoneum, and the adaptation of the edges still further secured by superficial sutures of silver, which were placed in the intervals between the pins. The pedicle was brought outside, and secured by passing a superficial suture through it and the integuments, to prevent its receding into the abdominal cavity. Strips of plaster were used to steady the ligature, and the skin was also protected from the edges of the pins, in the same manner. Strips of plaster extended across the abdomen, and the application of a pad of cotton-wool and a flannel binder, completed the dressing. The patient was put to bed, expressing herself as very comfortable, and "wishing to know if the tumour had been removed."

The tumour, exclusive of the contents of the cyst, which had been tapped, weighed sixteen pounds; the weight of the fluid contents of the cyst was twelve pounds; making altogether twenty-eight pounds, as the weight of the tumour in its entire state. It was a good specimen of the pseudo-colloid variety of ovarian tumour, so well described by Spencer Wells. An incision into it showed it to be made up of a number of chambers, varying in size from a filbert to that of the clenched hand, and filled with two different kinds of fluid; one kind presenting the appearance and consistency of very thick mucilage, the other resembling prepared chocolate; the parietes of each chamber consisted of a layer of firm fibrous tissue.

I made a careful microscopic examination of the fluid, but could find none of the morbid cells which are characteristic of malignant growth, but only a number of non-nucleated cells, aggregated together, so as to form corpuscles three or four times the size of blood-discs, and having the appearance of fatty granules.

An hour after the operation, the abdomen was covered with a linseed-meal poultice. At 5 P. M., after four hours' sleep, the patient awoke, and expressed herself comfortable, but fatigued, and rather sick. In the evening, she vomited a little,

the effect probably of the chloroform; at 11 P. M., a morphia suppository was introduced into the rectum; pulse, 104; skin, moist. She passed a good night.

Second, third, and fourth days.—Pulse ranged from 104 to 130. The morphia suppository was repeated each evening. She had no pain, no vomiting, and still expressed herself as being very comfortable; tongue, clean. Her diet consisted of Liebig's beef-tea, barley-water, gruel, and tea and dry toast.

On the second day, as the weight of the linseed-meal poultice caused discomfort to the patient, it was replaced by a chamomile-bag, i. e., a flannel-bag filled with chamomile, which had been boiled with water, and freed by pressure from superfluous moisture; the whole being covered with oiled silk, and repeated as often as it became cold. On the morning of the third day, the wound was dressed for the first time; and the dressing was afterwards repeated every day.

Fifth day.—Pulse, 125. At 10 P. M., she was suddenly seized with vomiting so intensely acid as to excoriate the mouth and chin, and so violent as to threaten speedy dissolution; this continued all the night, and the whole of the next day, up to 11 P. M. To afford relief, the following remedies were successively administered: bicarbonate of potash (which, she said, produced a burning sensation in the mouth and throat), carbonate of magnesia with prussic acid, chloric ether with aromatic confection, ice placed in the mouth, an injection of one pint of barley-water to move the bowels (which it accomplished), and, lastly, an injection consisting of one fluid drachm of tincture of opium, in one ounce and a half of water. The cessation of the vomiting appeared to be due entirely to the opiate injection, the preceding remedies having all been ejected from the stomach almost as soon as administered. The relief produced by the opiate injection was immediate, and, in three-quarters of an hour, she fell into a sleep, which continued for five hours. When she awoke, some beef-tea and a little iced brandy were administered; these remained on the stomach; and, in fact, vomiting never afterwards returned. It is necessary to state that, for some time after the operation, the catheter was regularly introduced every four hours.

Sixth and seventh days.—The wound appeared to have united in its deeper portions by the first intention, the superficial portion still remaining open; and there was a slight discharge of healthy pus. One of the pins was taken out.

There was no abdominal tenderness, nor tympanitis; the skin was moist; tongue, clean; appetite, moderately good; pulse, 116; bowels had been slightly moved. The laudanum

injection was repeated at bed-time, in two ounces of beef-tea. From this time she continued to progress very favourably, without any bad symptoms.

Two pins were successively taken out; and on the twelfth day the last pin was removed, the wound rapidly healing. The patient now took port-wine, beef-tea, champagne, brandy-and-water. The laudanum injection was still continued each night. Pulse averaged 104; bowels moved spontaneously once a day.

Sixteenth day.—Ligature came away along with a portion of the sloughing pedicle; pulse was 98; tongue, clean; surface, moist; there was no abdominal tenderness; the wound had healed, with the exception of a small point here and there, and the portion through which the pedicle had passed. The diet was more liberal from this day; the patient taking boiled chicken, mutton chops, and a little sole which she had fancied.

Seventeenth day. Progressing favourably, but complaining of a tickling cough, which was slightly increased towards evening; pulse 104. I prescribed six grains of citrate of iron and quinia, in a little water, three times a day.

Nineteenth and twentieth days. Pulse ranged from 100 to 108; appetite good, skin moist, bowels free; cough still continuing, and, as before, increasing towards evening; for relief, squills, senega, and morphia, were administered.

Twenty-second day. This morning the cough was more urgent; and, in addition, there was a loud mucous râle, owing to a very copious secretion of mucus in the right bronchus and its ramifications, as was evinced by auscultation; pulse small, frequent, thready; the appetite, from having been previously good, now suddenly fell off; profuse perspiration, and symptoms of general prostration supervened, and in the evening she appeared to be rapidly sinking. Beef-tea, brandy, and port-wine, were ordered to be administered. As the patient refused to swallow them, and as the necessity for stimulation was imperative, recourse was had to their free administration "per rectum." This was repeated every hour, in the following quantities:—beef-tea, two ounces; brandy and port-wine, of each half an ounce; mix.

After seven hours, as the patient had improved, the injections were diminished in frequency, but still administered every two hours; the quinia and iron being continued.

On the morning of the twenty-third day, she was much better, but suffered a slight relapse in the evening; the injections were continued at the same intervals during this day, and

her pulse rapidly increased in fulness and strength; the cough, instead of being constant, as heretofore, came on in occasional paroxysms only, the sputum being thinner, and the bronchial tubes clearer; and she was able to take more nourishment by the mouth. The injections of beef-tea and port-wine, however, were continued every two or three hours, as circumstances appeared to call for them, until November 28th, by which time her cough had entirely left her; at this time, also, the wound was healed in its whole extent; her appetite was good, she took solid food at every meal, and was rapidly acquiring strength.

December 2nd. She has continued to progress favourably, and was up to-day for the first time.

From the twenty-second day to the twenty-ninth, the period of the attack of bronchitis, stimulation was resorted to lavishly; the patient taking, by mouth and rectum combined, one quart of old port-wine, and half a pint of brandy, every twenty-four hours, in addition to which, as much beef-tea was given as it was possible to administer.

January 1st, 1861. Since the last report, the patient's progress has been satisfactory, she having been up every day for some hours. A lady, whose residence is situated in one of the most delightful and salubrious of the rural districts surrounding Manchester, moved by the sufferings, and interested by the happy recovery of this poor patient, has offered her the advantage of a month's residence in her house, for the more complete restoration of her health and strength. The departure of the patient for this kind lady's country seat has been delayed only by the prevailing severe weather.

I believe that this poor woman owes the preservation of her life to an adherence to the following principles, laid down by Mr. Spencer Wells, viz.:—

First. In making an incision the smallest possible compatible with the passage of the tumour. I may here remark, that all the cases I have seen in which the large incision, "from sternum to pubes," was used, proved fatal.

Second. In bringing and securing the pedicle, external to the incision, instead of leaving it to slough (as was the practice heretofore), within the abdominal cavity. It has fallen to my lot, on more than one occasion, to witness the death of patients about the third week after ovariotomy; the result, as there is the strongest possible reason to believe, of pyæmia,—the abdomen, on a post mortem examination, having been found filled with pus, which could be traced to no other cause than the sloughing pedicle.

Third. In the use of silver pins passed through the abdominal parietes, so as to include the peritoneum, thus securing union, by first intention, of the deep parts of the wound, including the divided edges of the peritoneum; and so, by closing the peritoneal cavity, securing it against the entrance of pus, which might otherwise fall into it from any superficial suppurating portions of the wound.

In my opinion, I believe, also, that part of the successful result is due to *the very free* administration of nutriment and stimulants, "per rectum," when the patient was unable or unwilling to swallow them, and at a time when death appeared imminent from the want of them.

I would draw the attention of my professional brethren to the remarkable extent which nutriment and stimulants are capable of being absorbed into the system when administered by this channel.

Great relief was also afforded by the substitution of a light chamomile-bag, covered with oiled silk, instead of the heavy linseed poultice; the latter having occasioned the patient much discomfort by its weight. I have already mentioned that the opiate injection was the only remedy which was successful in checking the obstinate vomiting.

In conclusion, I would wish to take this opportunity of recording the praiseworthy manner in which the patient was cared for, in my absence, both by day and night, by Mr. Runcorn, House Surgeon, and Mrs. Horncastle, Matron, of St. Mary's Hospital, whose indefatigable exertions have done much in bringing this case to a successful termination; the satisfaction derived from the remembrance of having so conduced to it being the sole, but ample reward, of their sacrifice of time, labour, and health.

Art. IV.—*An Essay upon the Malformations and Congenital Diseases of the Organs of Sight.* By W. R. Wilde, M.D., M.R.I.A., F.R.C.S., Surgeon to St. Mark's Ophthalmic Hospital, Honorary Member of the Medical Society of Stockholm, &c. &c. With Illustrations. Part III.

(*Continued from* vol. vi. p. 289.)

Circumstances have prevented my continuing those articles upon the malformations of the eye, which, the readers both of the former and the present series of this Journal may recollect, were commenced as far back as the year 1845. By reference to the number for November, 1848, it will be seen that

I concluded the article of that date with a description of the malformations of the iris and choroid. According to the plan originally adopted in the arrangement of this essay, we now come to the consideration of the congenital diseases and malformations of the dioptric media. The abnormal conditions of the fluid filling the aqueous chamber, irrespective of general disease of the organ, such as hydropthalmus, must be very rare in the human eye. I am not aware of any well-authenticated instance mentioned by the various authors whom I have consulted, of its being in greater quantity than natural, except that related by Benedict and Demours. Von Ammon mentions the instance of a calf, in which the anterior chamber was filled with a dirty red, gelatinous mass, and there was also a thickening of the membrane of aqueous humour; and Professor Prinz afforded Seiler the particulars of the case of a dog, in which, at birth, a movable body, with a polished surface and a sooty appearance, was found in the anterior chamber; but, as the author himself observes, it was, probably, an opaque lens, marked by the pigment of the uvea, which had started through the pupil[a].

Dr. France, of London, has recorded a case of congenital deficiency of the aqueous humour, and has been good enough to furnish me with a drawing of the patient, from which the accompanying illustration has been made. The corneæ were slightly convex, the irides bluish, and lay in contact with the

corneæ. There was no anterior chamber, and no aqueous humour; the irides were active, and free from adhesions; the pupils blue, clear, and circular. The subsequent history of this case, forwarded to me in 1850, is unknown[b].

MALFORMATIONS OF THE LENS AND CAPSULE.

The best authorities upon embryology, and more especially upon the formative process of the human eye, state that the

[a] Seiler's *Beobachtungen ursprünglicher Bildungsfehler und gänzlichen Mangels der Augen.* Dresden, 1833, s. 61.

[b] London Medical Gazette, vol. x., p. 11.

lens becomes apparent in the human fœtus about the commencement of the sixth week. Minute anatomists differ as to whether the capsule, with its contents, is a portion of the original internal or medullary tube, continued on into the ocular vesicle destined to form the future visual organ, or a reflexion of the external tegumentary envelope, drawn inwards so as to form the lens-capsule, and afterwards nipped off by the action of the iris and choroid.

It would not tend to elucidate the more immediate object of this essay, were I to enter into a discussion of the various and rival theories which have been promulgated during the last twenty-five years upon this subject. As already stated in the former portions of this essay, the *Encyclopédie Anatomique*—especially vol viii., containing Bischoff's *Entwickelungsgeschichte der Saeugethiere und des Menschen*, translated from the German by Jourdan, published in 1842—really contains the great bulk of all the authorities which could be quoted upon the subject. Some anatomists include the vitreous substance with the original formative process of the lens; and others define it as a mere mechanical recipient of that body. The former, however, seems the most plausible theory, and one justified by the subsequent intimate relation of these two structures. Huschke, one of the earliest and most accurate observers, while he believes in the connexion of the vitreous body with the primary medullary tube, holds that the lens is formed by a reduplication, or tucking-in of the tegumentary membrane. Those who assert that the lens-capsule is formed from without, produce, in proof thereof, the fact of the original want of chambers in the eye, and also, that at an early period of embryo existence the anterior section of the capsule is in immediate contact with the iris, and, so far as the membrana pupillaris admits, with the cornea also. It is even asserted that an opening exists (in the chick, for example, at the end of the third day) in the middle of the future cornea; but this has been denied by others. The substance of the lens is believed to be formed by, or secreted from, the inner layer of the capsule; and in the early period of fœtal life is composed of a mass of granules and globules of a much softer consistence than it subsequently assumes, when these globules, uniting in compressed lines or bands, form the fibres of which the future lens is composed. In consistence it is then soft, compared with all its future stages from birth to old age, and is of a reddish hue; but whether derived from the amount of surrounding vascularity (as from the capsule and the reddish vitreous body, &c.), or by any deposit of colouring matter within its substance, has

not been clearly defined by writers. Some say that the lens is originally opaque, while others assert that it is transparent. These are questions of much interest in investigating the causes of those peculiar appearances which present at birth, and form the more immediate subjects for description in this essay.

During the first months of fœtal life the lens is globular, like that in fishes, and larger in proportion to the size of the globe of the eye than in extra-uterine life. According to Arnold, the process of becoming transparent begins in the circumference, and proceeds towards the centre; and this is an opinion in which, from my own observations upon congenital cataract, I can fully concur. The late venerable Von Walther of Munich also maintained the original opacity of the lens, and asserted that cataract was an arrest of development while it was passing into the normal transparent stage; while Von Ammon of Dresden, of whose observations I have so largely availed myself in the foregoing portions of this essay, maintains that it is a pathological change in an originally transparent structure. About the period of birth the lens becomes relatively smaller, and less spherical, than in the fœtus.

The capsule, resembling in structure the elastic lamina of the cornea,—a description of transparent fibro-cartilage,—may be said to be that which gives form to the lens, as well as produces from its inner secreting surface the proper substance of that body. It also supplies it, in early life, with its proper vascular support, by means of the ramifications of the arteria centralis retinæ upon its posterior surface, and a venous connexion anteriorly with the membrana pupillaris; while upon its periphery, and for some distance over its anterior edge, a minute and intimate vascular connexion is maintained with the edges of the ciliary process. Judging from analogy, we may well suppose that the membrane of aqueous humour, which passes over the front of the capsule, affords a general vascular connexion to all those structures, both in the anterior and posterior chambers, with which it maintains anatomical relations. Intimate as these connexions of the lens with the surrounding parts of the eye appear to be in embryonic and fœtal life, they cease to be demonstrable after the full period of intra-uterine existence; so that, except by the fact of an adhesion of the lens to its capsule, when the latter is fully opened, we have no anatomical proof, either by injection or by the aid of the microscope, of any union between these two parts.

"As the crystalline lens in the healthy eye of an adult," says Dalrymple, in his "Anatomy of the Human Eye," "is perfectly transparent, and void of colour, it follows, of course,

that it contains within its substance no vessel carrying red particles of the blood; and inasmuch as in earlier periods of the science the art of making anatomical injections was unknown, it is anything but surprising that the existence of bloodvessels in this part of the eye was denied by Galen and the old anatomists. Haller, in his description of the arteries of the eye, quotes an Englishman of the name of Allen Moulin as the first observer, and in fact the discoverer, of these long-denied vessels." These vessels were, however, subsequently acknowledged by Ruysch, and demonstrated by Winslow. The "Englishman" alluded to in the foregoing paragraph was Allen Moulin, or Mullen, an Irish anatomist, and one of the original members of the Dublin Philosophical Society in 1684[a].

Ancillary to the foregoing observations may be considered the question of the restoration or regeneration of the eye, in whole or in part, as related by authors. The experiments and observations which have been made on the lower animals tend to prove that the contents of the capsule may be repaired after its removal or absorption. So early as 1781, C. Bonnet appears to have made experiments which proved that the eye of the newt, or salamander, could be restored, if the globe, as far only as the entrance of the optic nerve was removed[b]. Blumenbach made some observations upon the same subject subsequently[c]. In 1801, G. Vrolik, of Amsterdam, observed that a depressed lens had been imperfectly replaced, after ten years, by an irregular ring of lenticular substance, open in the centre, and which he thought arose from a new secretion of lenticular substance, owing to the incompletely-destroyed vessels of the capsule[d]. Twenty-six years later, Leroy D'Etiolles, whose experiments were chiefly made upon rabbits, cats, and dogs, and who was, perhaps, unaware of the foregoing discoveries of Bonnet and Vrolik, stated that the restored part was either a crumbly mass, a diminutive, lenticular body, or even a full-sized lens. Mayer's experiments demonstrated that in rabbits, eight days after the destruction of the lens, a small ring of soft lenticular substance had formed; and that this went on increasing in quantity until the middle of the fifth month after the operation, when a new lens, open in the centre, but normal in the circumference, was formed; but although it is not so stated, these so-

[a] See the Author's Preface on the History of Medical Literature, in the first volume of this Journal.

[b] *Œuvres d'Hist. Nat. de Philos.* Liv. xi., 1781.

[c] *Kleine Schriften.* Leipzig, 1804.

[d] See Müller's Physiology, translated by Baly; and also Magendie's Journal for 1827.

called lenses were, I presume, opaque. When the periphery of the capsule was destroyed, there was no reproduction.

In the details of all these experiments, the original mode of destroying the lens is not clearly expressed. The subject has been much discussed among the German physiologists; but the results of their inquiries and experiments have not tended to advance practical science—in this country, at least. William Soemmering's and Werneck's experience confirm the opinion of Vrolik, and show that after reclination, or depression, a crystalline mass is sometimes formed in process of time within the capsule, the posterior segment of which is alone open.

As, however, almost all these experiments and observations have been recorded by anatomists not practically engaged in operative ophthalmic surgery, and consequently not very familiar with pathological products in the human eye, I think I am warranted in asserting that we require satisfactory evidence as to the reproduction of the lens, in whole or in part, in man, under any circumstances. Those who have had much experience in operating know full well that, owing to the cortical substance of the lens being softer than the nucleus, especially towards its circumference, a portion of it often remains within the capsule, even after the operation of extraction. Unless anatomists are also experienced operators, their mode of treating the eye in the lower animals for the purpose of these experiments, may, I think, with justice, be questioned by those who well know the great difficulties which surround this branch of practical surgery. Even acknowledging to the fullest extent the most favourable results of these experiments, observations, and assertions, no practical good is ever likely to accrue therefrom, no more than from the asserted transplantation of the cornea, or, as stated, the reading with the pit of the stomach, as some years ago was gravely stated to me by an eminent dignitary of the Irish Church[a].

Deviations from the normal condition in this part of the organ of vision may be divided into:—the absence of lens; the

[a] Vagaries of this description, so repugnant to common sense, irrespective of science, and so subversive of the cause of true religion, might be laughed at in the ignorant, and despised in the charlatan; but when they are upheld by the exalted in station and influential in position, they become truly dangerous; and when they bias a mind, by nature "open as day to melting charity," so far as to withdraw from the public charities of a large city, which supply shelter, food, care, and all the appliances which science and humanity have devised for the amelioration of human suffering, because mesmerism and homœopathy are not practised in such institutions, then there can be no hesitation in asserting that such conduct becomes not only deplorable, but reprehensible.

fusion of two lenses together, as in monoculi; a double lens in one eye; malpositions of the lens, and alterations in the size, form, structure, consistency, and transparency of the lens and its capsule, as well as organic changes, such as absorption of the lens, and adhesions between the iris and the anterior capsule, from intra-uterine accident, or disease; and, finally, entozoa within the capsule, &c.

Absence of Lens.—Rosas quotes Morgagni, Von Walther, and Arnemann, as authorities for cases of deficiency of the lens[a]; and Haller records the case of a child with but one orbit, containing, however, two globes, but not fused, as in monoculi. One globe was perfectly normal; in the other, however, the lens was absent[b]. Seiler has recorded a case of deficiency of the lens in a microphthalmus. A similar case is recorded by Von Ammon; but it is probable that in both instances the deficiency was the result of the general disease, or arrest of development in the whole organ at a very early period of fœtal life. In case of deficiency of lens, one would expect fluctuation and unsteadiness of the iris.

Double Lenses.—Supernumerary lenses have been observed by Vallisnieri and Fritsch; but the particulars are not specified in the works to which I have had access; and it is possible that these cases may have been instances of cyclopia, in which two eyes were fused together. Wardrop says—"The crystalline lens is sometimes entirely wanting; in which cases the pupil is also double"[c]. But he does not cite his authority, nor more particularly describe the cases. He adds, however, that Heister "found it [the lens] divided into different portions," or fused lenses. See *Monoculi.*

Myopia, arising from abnormal curvatures of the anterior or posterior capsule of the lens, must frequently be the natural or born condition of these parts, although not remarkable until the child has arrived at that period of life when its short-sightedness attracts attention. Authors state that congenital cataract has been confounded with, and occasionally mistaken for, myopia. I have more than once seen such a case. Porterfield says the lens has been, in some rare instances, unusually flat; and hence the patient was congenitally presbyopic, or long-sighted. And Foigtel states that it has been found of a triangular shape.

Malposition of Lens.—Klinkosch asserts that he saw a lens

[a] *Handbuch der theoretischen und praktischen Augenheilkunde von Anton Rosas.* Wien, 1830, I. Band Seite, 284.

[b] *Hist. de l'Acad. de Science*, 1751, p. 49.

[c] Essays upon the Morbid Anatomy of the Eye, by James Wardrop, vol, ii. p. 76.

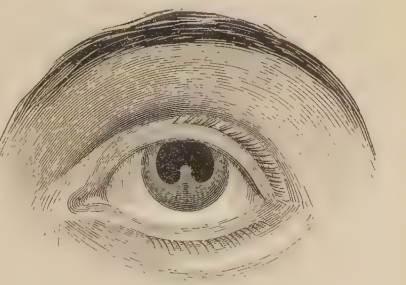
behind the vitreous body, and lying on the optic nerve. Such a case is not by any means improbable, when we consider how easily it might occur in case of fluidity of the vitreous humour. Although anterior dislocation of the lens, observed at birth, has not been related by authors, it is an accident so likely to happen, that one rather wonders at the absence of it than otherwise.

Mr. Dixon, in the last edition of his "Guide to the Practical Study of Diseases of the Eye," records the circumstance of the "abnormal position of the lens occurring in four members of the same family," in which the lenses appeared to have had one edge tilted forwards, and the other backwards, either from side to side, or from above downwards. In one instance, however, the twist of the lens took an oblique direction[a].

Entozoa within the Capsule at birth have been observed and described by several authors. Von Ammon has figured two forms of *Distoma*, and also a *Filaria oculi humani*, found in connexion with congenital cataract; but does not state the particulars. Nordmann found entozoa between the lens and capsule in the embryo of fishes. The same author, Gescheidt, and other observers, have related many cases of entozoa within the capsule after operations performed for non-congenital cataract[b].

The lens has been found absorbed, and the capsule shrivelled up, apparently from defective nutrition, during embryo life.

Congenital Synechia Posterior, the result of intra-uterine iritis, occasionally occurs, as I have shown in the previous portion of this essay. It is a pathological condition which I have myself observed so soon after birth, that it could only be attributed to inflammatory, and probably specific action going forward in utero. The case illustrated by the accompanying woodcut is, however, of a different character from those instances of syphilitic intra-uterine iritis which are usually met with. The pupil, here represented under the influence of belladonna, is horse-shoe-shaped, owing to attachment of



[a] See Appendix to the Second Edition, p. 401.

[b] Upon this subject see *Zeitschrift für Ophthalmologie*, band III. heft 4; and article "Entozoa," in the Cyclopœdia of Anatomy and Physiology, by Professor Owen; also *Archiv für Ophthalmologie*, Bd. i., iv., vii.

a small portion of the free edge of the iris to almost the centre of the lens-capsule; this appearance existed at birth. Vision was quite perfect when I had this drawing made, in 1849, from a young woman then aged 23.

Congenital Cataract may be divided into capsular, lenticular, and capsulo-lenticular, according to the structures affected, —partial or complete, according to the amount of opacity present; fluid, or of the normal consistence of the lens; and single or double, as one or both organs are affected; in all of which this disease is analogous to those forms observed in after life. But there are two descriptions of cataract which are solely the result either of intra-uterine *disease*, or that occurring immediately after birth; these are the central and the pyramidal.

Until the days of Saunders, English surgeons do not appear to have paid much attention to congenital cataract, neither had it been well described by Continental authors. To Saunders is also due the introduction of the early anterior operation for this affection, or what he properly terms the operation on the capsule. Since then, German writers have been very minute in their definitions of congenital cataract; thus, Himly describes *Cataracta capsularis totalis, anterior, posterior, et centralis; C. lenticularis dura, caseosa, lactea, et centralis; and C. capsulo-lenticularis dura, centralis, et pyramidata.* Among the forms of cataract described by Von Ammon, including all the foregoing, he enumerates those connected with the malformations of the iris, club-foot, umbilical hernia, and elongated crania, &c.

The most frequently attending symptom of congenital cataract is nystagmus, which is more or less decided, according to the density of the opacity, or the space it occupies behind the pupil, and occasionally aggravated by amaurosis. The two forms of motion of the globes may, however, be distinguished by a careful and experienced observer. Where the irregular motion of the eyeball has been caused or aggravated by nervous disease, there is generally a staring look in the eye, and the motions partake more of the rotatory form than those more side to side; while in the uncomplicated cataract, with a healthy brain and retina, the movements consist of a "peering" upturn of the eye, at the same time that the features of the face are brought into what may be termed a seeing or listening attitude; occasionally, the globes take sudden transverse sweeps within the palpebral aperture, as if seeking light, while the head is usually turned a little to one side. According to my observation of congenital cataract, it is very rarely connected with any other malformation. In two instances, however, both in men past middle life, and affected with double congenital cen-

tral cataract, each consisting of a dense, white spot, about the size of a large pin's-head, there was a remarkably high formation of skull,—what the Germans would call "Spitzkopf," and not unlike some of those malformed heads figured by Von Ammon, in his tables of *Cœcitas Congenita.*

I lately witnessed a case of congenital cataract, with deficiency of the iris on both sides; but as the girl had been operated on prior to my seeing her, I cannot state what was the original condition of her lens and capsule. She had intense nystagmus, and great intolerance of light. I am inclined to think such cases should not be interfered with. Tremulous iris occasionally coexists with the born cataract, and the lens itself is occasionally shakey.

The causes of congenital cataract may be divided into the proximate and remote. The latter is more frequently induced by hereditary predisposition than any other affection of the eye with which I am acquainted. I also believe that it may, like muteism, be induced by the consanguinity of parents; but we require more extended observations to confirm this opinion. All authors, however, agree with respect to the hereditary character of the disease. I saw three generations affected with it, and I operated upon members of two of them. All the older authors, from the days of Maitre Jean and Woolhouse, were well aware of the hereditary nature of this disease. The journals and works upon ophthalmology record many curious instances, illustrative thereof. I have several times seen three of a family affected with it; and I know an instance in which the father and his six children were born blind from this cause. The father had been operated on by the late Dr. Little, of Sligo; the three children whom I examined had all malformed eyes, in addition to the opacity of the lens; two were totally amaurotic, and the third had a microphthalmus on one side. The form of cataract, whether central, lenticular, or diffused, &c., is generally persistent in the different members of the family, although I have seen exceptions thereto. Saunders relates the following remarkable case, showing the identity of the disease in the same family:—"Two brothers, twins, became blind with cataracts at the age of twenty-one months, each within a few days of the other. It is remarkable that the four cataracts had precisely the same character." In these instances, however, it is more likely that the children were born with cataracts.

Before I come to discuss the proximate cause of the diseases, I wish to dispose of one form of non-congenital cataract, with which it is often confounded. Under the head of congenital cataract, authors have described central capsular cataract, caused

by ophthalmia neonatorum; but they have not explained the mode in which this occurs. Infants become affected with purulent ophthalmia a day or two after birth; in many cases the cornea opens in the centre, thereby differing in some respects from the violent ophthalmia of adults, in which that structure most frequently gives way near its circumference. Possibly, the cornea is weaker in the centre at birth than subsequently. The rent having taken place, either by slough, ulceration, or what might really be styled a crack or burst, the aqueous fluid is evacuated, and the iris and lens fall forward against the cornea, where the capsule, by simple apposition with an ulcerated, or otherwise diseased surface, becomes, in my opinion, inflamed and opaque by mere contiguity of parts. The rent in the cornea closing, by lymph-deposit on its edge, or becoming plugged up with a portion of protruded iris (in which case there is permanent synechia anterior), and the fluid re-accumulating, the chambers are re-formed, and the lens is restored to its normal position, but with the central opacity remaining permanent. It is always in one eye only, and is often connected with the anterior synechia, as shown in the accompanying illustration, taken from a man then aged 25, and with whose history I am acquainted.

The proximate causes of opacity of the lens may be stated as arrest of development, during the clearing process, supposing the lens to have been originally opaque—interstitial deposit of opaque matter occurring in utero as in after life—inflammation producing opacity especially of the capsule; and morbid growth, as in the case of anterior pyramidal cataract. According to Arnold, the lens-substance is originally a thick milky fluid, which subsequently acquires the consistence of albumen, then becomes more solid, and finally begins to clear at the circumference, leaving the central nucleus opaque, as in the fifth week in the fœtal calf, and does not altogether become transparent till the eighth week.

Werneck found the human lens opaque up to the eleventh, and in some instances to the fourteenth week. Thus, an arrest of development at the very earliest stage of embryonic life would afford at birth the milky cataract; and, at a little later period, that of the general homogeneous opacity which is a

common form of congenital cataract; although, I believe, it may also be the result of inflammatory action, or congestion. The vices of formation receive more distinctive proof from that form of cataract which Dr Mackenzie calls *cataracta cum zonula*, in which there is a central nucleus of opaque matter surrounded by an envelope of transparent lens, through which occasionally run spiculæ, or needle-shaped opacities. Other authors, and among them, Haller, Zin, Petit, Seiler, and Von Ammon, hold that the lens is originally clear, and that the deviations from the normal formative process are owing to disease in the capsule or lens, and more particularly to obliteration of the arteria centralis retina. This is susceptible of explanation by analogy with a not uncommon accident in after-life—concussion of the lens from a blow on the eye, the brow, or the temple, when the surrounding vascular connexions of the crystalline being destroyed, it becomes, in an incredibly short space of time, opaque. But although this explanation may account for the diffused form of lenticular cataract at birth, it does not elucidate the circumscribed and partial opacities of congenital cataract. Scrofula is also charged with this congenital defect; but in what way it acts has not been explained by the supporters of that doctrine. In an inquiry of this nature, nothing very certain or exact can be affirmed: and, even if it were, it could exercise but little practical effect. The forms, however, which the disease presents are of far greater importance to the ophthalmic surgeon.

It is asserted by Wardrop and other writers, that although congenital cataract commonly affects both eyes in the same way, that some times the lens of the one and the capsule of the other is affected; but this is contrary to my experience; for where the cataract was truly congenital, I have always observed a marvellous amount of symmetry between the eyes. It is true, a slight speck on the anterior capsule may be observed in connexion with lenticular cataract in one eye, and not in the other; but it is not sufficiently extensive to be denominated capsular cataract, according to the sense in which it has been applied by Wardrop. Having paid a great deal of attention to this subject many years ago, I had a very extensive collection of drawings illustrative of the different forms of the disease (many of which are not figured in books), made by Mr. Connolly and the late Mr. Neilan; but without coloured engravings, it is very difficult to represent their special peculiarities.

Distinguished from all other forms of cataract, the congenital, which is the result of some vice of formation, almost invariably presents a definite, distinct, well-defined figure, not

shading off gradually into the surrounding clear portion of the lens, as in the opacity of age, nor mottling the lens irregularly, like that of middle life, but presenting a sharp line of demarcation between the opaque nucleus and the surrounding normal transparent structure.

Lenticular congenital cataract may be divided into the complete and partial.

Complete Lenticular Cataract not uncommonly presents at birth, in which the capsule is unaffected, and the entire substance of the lens is of a uniform, skim-milk colour, homogeneous, and retaining, so far as we can judge, something about its normal consistence, perhaps a little softer. I have seen this transmitted more than any other form, although it may occasionally appear accidentally in one member of a family. Owing to the great occlusion of light, there is always nystagmus in such cases; and they are those which usually require early operation. Von Ammon relates the case of a hard lenticular cataract in one eye, and a soft lens, with partial opacity of the capsule, in the other. I have seen one instance (to be described hereafter) in which the lens was yellowish, and apparently hard in one eye. In two cases of congenital cataract, also recorded by Von Ammon, the yellow spot on the retina was wanting—as shown, I suppose, by dissection.

Cataracta Lactea, in which I presume the lens has either remained in its original fluid and opaque condition, as already stated, or, having become solid and opaque, has degenerated into the milky substance observed at birth, like the softening process which the cortical substance of the lens undergoes in what is termed Morgagnian Cataract, is rather rare. In all the cases of this kind which I have seen, the lens appeared unusually large; there was great nystagmus, and a certain amount of amaurosis, with dilatation of the pupil, present; the capsule is frequently mottled with greyish-white spots. This form may be distinguished from the foregoing variety by its less apparent density; and, when the capsule is mottled, by the greater amount of relief with which such opacities are thrown forward. But it requires a very practised eye to make an accurate diagnosis of the amount of fluidity in certain cases of cataract. One of the most remarkable circumstances attending this pathological condition is, that while the colouring matter of this milky substance remains either well mixed or suspended in the fluid, so long as the capsule is intact, when it is opened, as by the needle in operating, and that the whitish substance mixes with the contents of the aqueous chambers, it gradually, but within a few minutes, deposits in

the form of a greyish-white powder, leaving the pupil perfectly clear; as if the admixture of the aqueous humour had chemically acted upon it, and thrown down the precipitate referred to. Occasionally the substance which is deposited in the bottom of the anterior and posterior chambers produces, like that observed in Morgagnian Cataract, great constitutional disturbance, with irritability of stomach, and inflammation of the eye.

Neither of these two forms of cataract could be exhibited effectively in a woodcut.

Central Lenticular Cataract.—I have been for many years acquainted with a peculiar form of congenital cataract, to which this appellation is applicable; and I presume it is that known to some of the older writers as the *cataracta cum zonula*, but which name Dr. Mackenzie applies to that form of siliquose capsulo-lenticular cataract, in which the lens and capsule, "not having grown in proportion to the rest of the body, but remaining nearly of the size they were at birth, on dilating the pupil, a black zone—formed exteriorly by the ciliary processes, and interiorly by the space between them and the circumference of the capsule—is brought into view, surrounding the cataract." The disease, however, of which I speak, is purely lenticular, and unattended with any alteration in the form or deficiency of size in the crystalline. It is so frequently met with in this country, that having a large collection of drawings of both eyes (in 13 cases), made of it many years ago, and representing all its varieties, I have long ceased to illustrate such cases when they present. The only author who has presented us with anything like a faithful representation of the disease is Sichel, in his *Iconographie Ophthalmique*, pl. xviii. figures 1 & 2. In Dalrymple's Pathology of the Human Eye, Plate xxvi, figures 4 and 5 would appear from the drawing to be of this nature; but the author, no doubt looking upon the central opacity as including the entire substance of the lens, believed that the crystalline was "manifestly smaller than the natural structure"—an opinion to which Sichel also leans.

The accompanying woodcuts serve, as well as such illustrations possibly can, to illustrate this form of the disease, but they fall very far short of giving anything like a characteristic expression of the appearances. On looking into an eye so affected, we observe an opacity, more or less dense and whitish, behind the pupils; but at such a distance, and with so much shadow thrown from the pupillary edge of the iris, as to show a practised eye that a considerable clear space intervenes between the level of the pupil and the surface of the opacity. On

dilating the pupil, we then observe a clear, sharp, well-defined, and always circular edge to this opacity, round which there is a perfectly clear, and consequently black zone, varying in size according to the magnitude of the central opacity, which usually occupies about three-fifths of the transverse diameter of the normal lens—the entire cortical substance of the lens, both in its antero-posterior and transverse diameter, together with the capsule, remaining perfectly clear. To an eye long accustomed to such examinations, this piece of opaque matter presents somewhat the appearance of the coloured enamel seen in the centre of some of the specimens of old Venetian glass, an art latterly revived in our globular paper-weights. The patient always experiences considerable improvement from the dilatation of the pupil; andit is a description of cataract which, when in a mild form, is so frequently mistaken for myopia, that it is often first observed in adults, having been in early life attributed to that affection[a].

In the early cases of this disease which I met with, I naturally experienced some difficulty in diagnosis, but the first on which I operated, proved the correctness of the opinion I had formed; for as soon as the capsule and anterior layer of the lens were well incised with the cutting edge of the needle, the latter became, in a very short time, opaque throughout, as in ordinary cases of traumatic cataract[b].

This form of cataract is often complicated with opacity of the capsule; and the disease itself, in its uncomplicated form, presents many varieties. The opaque nucleus is seldom uniform in colour and density throughout, although I have seen it occasionally so. The edge is sometimes whiter, and apparently denser than the portion immediately within it, and occasionally presents a very remarkable whitish ring. With this form, as shown in some of these illustrations, there is a central

[a] Several years ago I operated upon two ladies and a gentleman in one family, all upwards of twenty years of age, where the disease had not been discovered until the time I saw them. Their defect of vision was attributed to hereditary short-sightedness—their mother having very defective sight. Not long since, while examining a lady's eyes, I was struck by the peculiar moping gait and peering look of her daughter, a girl of seventeen or eighteen years of age, who was in the room; and, on inspection, I found she had congenital cataract in both eyes. On asking the parents about her, I received the following answer: "Oh yes, indeed, poor thing, it is a great pity; she's very blind, and we have gone to a great deal of expense in trying to get her proper glasses." The poor girl herself complained bitterly of the treatment she received at school, on account of her "stupid blindness." Neither the school-mistress nor the parents had ever thought of having her examined by any competent authority. She has since continued to use atropine, and derives so much comfort from it, that she is unwilling to risk an operation.

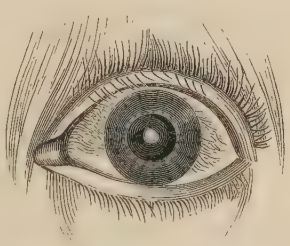
[b] Persons about to submit to an operation for this form of cataract should be informed previously of the result of the first necessary step towards solution.

spot denser than the portion between itself and the ring; and occasionally this part assumes a very regular figure, as seen in the right eye of William Nightingale, a boy aged 18, from whom I had this drawing taken, prior to operation, in 1847.[a]

Sometimes, instead of the appearances there presented, we may observe a little nucleus of white dots, each about the size of a small pin's-head, and which may be distinguished from the surrounding skim-milk colour of the opaque portion of the lens, as shown in the accompanying illustration of Mr. K.'s eyes.

In one instance the spot was so small, white, and central, as to resemble the cataracta centralis, which has its seat in the anterior capsule of the lens; and it was only by examining the eye in partial profile, that the difference could be distinguished. In two cases, I have seen lenticular cataract of a straw-coloured yellow, with a central anterior knob of dense white, like chalk. Both instances were adults, and the lenses were possibly hard; but the defined form of the opacity luted into the centre of the clear lens in each left no doubt as to its congenital origin.

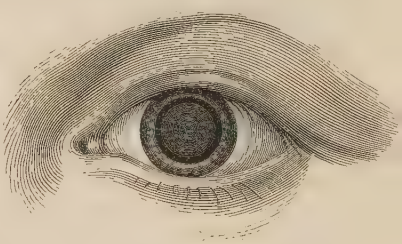
In the accompanying illustration, drawn from Bridget Danass, a woman, aged 46, both the draughtsman and engraver have been particularly fortunate in hitting off the aspect of the disease, and in showing the spaces that exist between the edge of the iris and the central nucleus of yellowish opacity of the lens on the one hand, and on the



[a] These blocks, as originally engraved, being too wide for the page, have been cut in the middle.

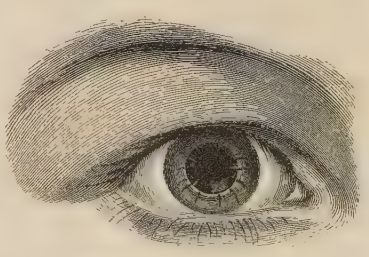
other the chalky-white anterior central cataract within the capsule, which throws its shadow upon the yellow opacity behind it. In the remainder of the lens, the interspace between the capsule and the congenital cataract was clear. The upper lid was affected with trichiasis, as shown in the illustration. It was for the latter disease the patient came under my observation.

Not an uncommon variety of this form of partial lenticular cataract consists in white striæ, radiating from a central spot with



extreme regularity, as shown here in the eyes of Louisa Pentony, aged 12, who was operated on in 1849. I may here mention, that regularity of formation affords, in most of these cases of congenital cataract, a very ostensible differential diagnosis.

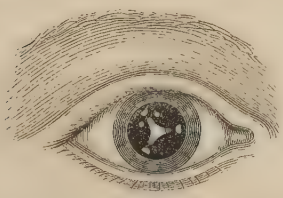
Starting from the edge of the opacity, we occasionally perceive a number of white lines passing out to the circumference of the lens, through its clear annulus, not unlike those spiculæ of opaque matter which are often the first manifestations of cataract in after-life, and are chiefly situated on the posterior surface of the lens, having their base at the extreme edge, with their apices pointing to the centre. Like these, the radiating lines in the pheriphery of the lens in congenital cataract, chiefly occupy its lower half, although a few are occasionally seen all round. Sometimes the radiating lines of the cataract are continued on into those peripheral ones; and in the accompanying drawing we see, in addition thereto, three opaque spots very distinct, and occupying the clear peripheral space in the normal lens. In his description of Plate xxvi, figure 4, already alluded to, Dalrymple has given a figure of what would appear to be a partial central opacity of the lens, although he was evidently aware of its surrounding normal investiture. In that repre-

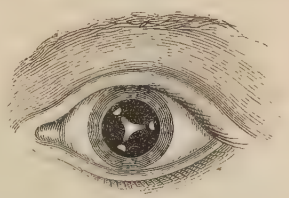


sentation, we observe two of those radiating lines, upon which he observes—"Sometimes we see a little opaque tag or fibre, which seems to attach the capsule to the vitreous body, or, more probably, to the anterior layer of the zone of Zinn; as if, the lens not increasing with the general growth of the rest of the eye, its former natural connexions had been partially separated, leaving, here and there, points of adhesion, which retain it in its central position." From our knowledge of the pathology of the eye, we could scarcely expect to find a dwindled lens within a clear normal capsule. The foregoing is one of the very few instances in which that observant author failed to detect the true character of any appearance within the human eye.

In connexion with central lenticular cataract in the left eye of Anne Duffy, aged 13, the appearance shown in the accompanying cut was observed when the pupil was artificially dilated. It resembled a curtain, with a fringed upright edge, stretched across and behind the iris. It was neither a development of the ciliary process nor a persistence of the membrana pupillaris. In colour it was light brown below, and whitish at the fimbriated margin. I have not seen the case since the drawing was made, in 1846.

When I commenced the revision of this portion of my essay, I thought I was acquainted with every form which congenital cataract could assume; yet here is the appearance in the eyes of a child, aged 4 years, who was brought to me last





week, and which, so far as I know, has never been observed previously. In the centre of each lens there is a whitish cruciform mass, the outline of which presents an extraordinary amount of regularity. Towards the extremity of each limb of this

figure, and somewhat deeper in the clear substance of the lens, may be seen small triangular pin-head opacities, and near the upper and outer limb on the right side, there are three white specks. Vision is imperfect, but the child is well-formed and healthy.

Very rarely, indeed, do we observe a small circular opacity in the centre of the lens; and when we do, it is very difficult to give it the proper expression. The accompanying drawing is the nearest approach to it which an illustration in wood can effect. Seiler says, that although the central cataract is usually white throughout, he has seen it annular, with a transparent spot in the middle; and he has figured this appearance in the plate attached to his work already referred to, see figure xi.

Posterior Lenticular Cataract.—Opacities upon the posterior capsule of the lens, if not often seen by some of the older authors, were at least believed to be of frequent occurrence. Many years ago, I observed a yellowish spot, with a few radiating fibres, very deep, in each lens, in a middle-aged man. I had a very careful drawing made of the appearance which the eyes then presented. In the course of about three years, the entire body of each lens became opaque. I extracted them, and demonstrated to those present upon each occasion, the opacity upon the apex of the posterior surface of the lens; and, when the section of the cornea had healed, showed that there was not the slightest opacity remaining upon the capsule. The man worked many years subsequently as a shoemaker. Since then I have seen many such cases, and never failed to recognise them as diseases of the posterior surface of the lens.

M. H., aged 15, with impaired vision from birth, presented the following appearance:—A dense brownish-yellow opacity,

circular in shape, larger in the left eye than the right, occupied the apex of the posterior surface of the lens. Surround-

ing each, there was a slight nebulous condition, with a well-defined edge not easy to represent on wood. The operation for solution was performed in both eyes, without any untoward circumstance; but although he is greatly improved, and now able to follow an out-door occupation, he is not able to read, and has derived less assistance from glasses than any case of congenital cataract I have ever met with.

A very natural suggestion here presents itself, and one which is sure to be asked by the partially blind from congenital cataract, or their friends—Will the disease increase, either in density or extent, unaided by any adventitious or accidental causes—such as congestion, inflammation, age, &c.? I believe not. I have never seen, nor have I read of, a well-authenticated instance of congenital opacity of the lens, or its capsule, increasing in after-life. Dr. Mackenzie, in the last edition of his great national work on the diseases of the eye, at page 769, when discussing the question of operating early in cases of congenital cataract, says—"If the operation is delayed, the capsule becomes opaque and tough, and, therefore, much more difficult to remove, &c." This opinion is contrary to my observation and experience of the disease. I never saw a capsule, unaffected at birth, become spontaneously diseased subsequently; nor, as already stated, have I ever seen a congenital opacity of the lens spread or increase in after-life; and in this respect the congenital differs from all other forms of cataract, either lenticular or capsular, except that which is the result of ophthalmia neonatorum.[a] There are, in many cases, cogent reasons for operating early, such as nystagmus, and total defect of vision, &c.; but the danger of thickening, and opacity of the capsule, does not, so far as I have seen, come under this category.

Capsular Cataract.—I have never seen the capsule so completely opaque at birth, as to obscure the view of the lens beyond; neither have I ever seen, through a transparent lens, an opacity on the posterior capsule. The most frequent form of this disease is a small white speck in the middle of the anterior capsule, and generally known as *Cataracta Centralis.* As already stated, it may exist in two forms, with or without lenticular opacity. When seen alone, it is often but the size

[a] I think it possible that the greyish-white nucleated cataract might, in after-life become both yellowish in colour and hard in substance, although I have no authority for so stating. Besides those cases related at page 65, the only instance presenting such an appearance I ever saw was in Thomas Cartmill, aged 36, where the substance of the opacity was a straw-coloured yellow, with a whitish annulus, and a chalky-white central nucleus; yet it is possible that these may have been the appearances at birth.

of the smallest pin's-head; and when congenital, and not the result of infantile ophthalmia, it almost invariably presents a very regular circular form, with a sharp, well-defined edge. The spot is always of a dull flake-white, but is occasionally surrounded with a greyish-white border, evidently of thinner material. This annulus is often so very narrow, as only to be observed with a good glass.[a] When the disease is the result of ophthalmia, this nebular ring is not seen, and the edge of the opacity is generally irregular. Many years ago, I observed that several of these central cataracts were nail-shaped, the head presenting anteriorly, and the tang—sometimes about a line in length, and tapering off to a point—passing into the clear substance of the lens, in which it could be well observed when the eye was viewed in partial profile. Upon examining some modern German publications, I find that this appearance has been denominated posterior pyramidal cataract.

With this species of cataract there is usually a good amount of vision, but varying according to the light; therefore persons so affected are improved by the application of belladonna. The disease has long been recognised by oculists, and was known even in the days of Taylor and St. Yves. Von Ammon is of opinion, that it may be caused by adhesion of the pupillary membrane and anterior wall of the capsule at that point. True congenital central opacity of the capsule is usually double.

Pyramidal Cataract is a large central opacity of the capsule, which bulges out beyond the level of the lens, and through the pupil into the anterior chamber. It is always conical, with a circular base, and presents a chalk-white appearance. Fourteen years ago, the late Mr. Hocken, of Manchester, was good enough to forward me a drawing, from which the accompanying illustration was made. Although not very artistic, it affords a good idea of the appearance, viewed in partial profile. Upon the card containing the drawing is the following, in Mr. Hocken's handwriting:—"Congenital Disease:—Conical opaque growth from the crystalline capsule, projecting through the pupil into the anterior chamber." He had promised to afford me the history of the case, when, unhappily for the cause of science, he died. Von Ammon who has paid a good

[a] By far the best glass I have ever used is the little compound instrument made by Hawes, of Leadenhall-street, London.

deal of attention to the subject, accounts for this disease by a patency of the presumed aperture in the anterior capsule in very early life, through which, he thinks, the substance of the lens bulges. He has also given drawings of the *post-mortem* appearance in an eye affected with this peculiar malformation; but the preparation appears to have been much altered by the spirit when the drawing was made. Wardrop figured and described a case of it, and said that Wenzel related a similar one, but where I have not been able to discover.

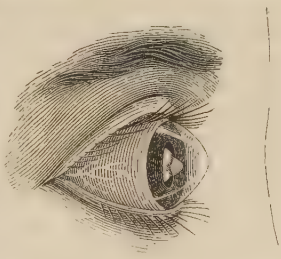
The latest record of pyramidal cataract in this country is that afforded by my assistant, Dr. Wilson, who says:—

"Pyramidal cataract, of which there appear to be two varieties, capsular and capsulo-lenticular, is characterized by an opaque-white cone or pyramid, its base being in or on the lens or its capsule, and its apex projecting into the pupil, and passing beyond the plane of the iris into the anterior chamber. Beer, who was, I believe, the first author to describe this form of cataract, said it was always the product of very acute inflammation of the eye-ball attacking principally the capsule, lens, and iris, and that it was always adherent to the pupil, which was immobile and irregular[a]. Beer gives no opinion as to its being congenital, but says it is formed not only by increase in volume of the anterior capsule, but also by a deposition on it of fibro-albumen, which can readily be separated from the capsule; and in this he is borne out by more modern writers, as Mackenzie and Dixon, two of the few English authors who mention this description of cataract. Some writers affirm that it occurs during the progress of ophthalmia neonatorum, when the cornea is swollen, and its posterior surface nearly in contact with the lens; this opinion is held also by Sichel, who gives an illustration of it in the 24th plate of his *Iconographie;* he admits, however, never having seen it follow the ophthalmia of infants, or found any opacity, &c., of the cornea.

"M. F., aged 17, a delicate-looking girl, seemingly much younger than she states, is small in stature, being only four and a half feet high; the head is flattened at top, forehead massive in proportion to face; hair dark brown; lashes very long and black; irides differing in colour. On examination, the following appearances in the right eye are seen. The cornea is altered in shape, being conical, and perfectly transparent; iris of a light-brown colour, having a washed-out appearance, rings of iris very faintly marked; the pupil is circular, and dilates and contracts naturally; no adhesions ap-

[a] Lehre von den Augenkrankheiten, 1817, bd. ii.

parent; it is occupied by an opaque lens, which, viewed in profile, presents the remarkable appearance I have delineated in the accompanying cut. The cataract involves the whole of the lens, and in front assumes, with the opaque capsule, a pyramidal shape, and protrudes into the anterior chamber; it is composed of several masses differing in colour and consistency; the apex of the pyramid is formed of a soft-looking substance of a blue-white colour; there is a sulcus between it and the base, which latter is apparently firmer; a mass of small opaque-white earthy-looking bodies projects from the temporal side of this latter portion. These are the conical bodies mentioned by writers as easily detached; in the present instance, however, they are situate posterior to the plane of the iris. On the nasal side there is what appears to be a membrane extending from the margin of the opaque lens, to which it is attached, towards the ora serrata; this is, undoubtedly, a portion of the ciliary processes.



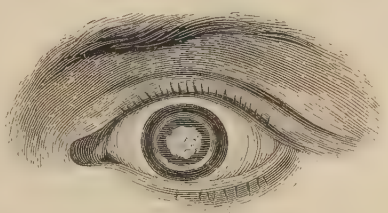
"I could not perceive, on ophthalmoscopic examination, any light reflected from the choroid. The girl has merely perception of light with this eye. There is also present in both eyes a symptom which is generally seen in congenital affections of the eye, nystagmus, or oscillation of the globe, an affection which has been made the subject of a book by Dr. Boehm,[a] who has proposed to cure it by cutting the internal rectus; a proceeding which will not, I believe, be undertaken by many practitioners. Conical cornea is, according to Von Ammon, frequently congenital, and often found combined with cataract."[b]

Capsulo-Lenticular Cataract.—In most of the cases which I have seen, there was either a central flake, of a dense dull white, and sometimes greyish, or yellowish-drab-coloured, and irregular in form, occupying about a sixth of the central or lower portion of the capsule, and contrasting well with the bluish-white lens behind; in other instances, especially in fluid cataracts, there were several small patches of white upon its anterior surface. In neither of these cases is it possible to discover the precise condition of the posterior segment of the capsule. In the former, that of a single patch of dull greyish-white sub-

[a] *Der Nystagmus und dessen Heilung*, von Dr. L. Beohm, 1857.
[b] Dublin Hospital Gazette for December, 1860, vol. vii. p. 372.

stance occupying the centre of the anterior capsule, I have found, in operating, that, when touched with the needle, it chipped off like a piece of eggshell; and in one case, that of J. Mooney, here figured, the scale fell into the anterior chamber, and remained there, innocuous, for more than a year, during which time the boy was under my observation. I have observed that there is less regularity between the appearances of the right and left eye, when the capsule is affected, than the crystalline. In the uncomplicated capsular cataract,

the opacity, whether merely central or pyramidal, is always regular both as to shape and position; while in the capsulo-lenticular, such opacity, or opacities, are irregular, both as regards their situation, shape, and colour, and would appear to be more the result of intra-uterine inflammation, than the consequence of any interference with the formative process. In the accompanying illustration of the eyes of Richard Lynn, a boy twelve years of age, made from a drawing taken when the pupils were dilated with belladonna, we have a good representation of one of the most frequent forms of congenital capsulo-lenticular cataract. In the original, the dull flake-white, irregularly-edged, and uneven surface of the large patch of opaque capsule contrasts well with the greyish-white substance of the lens beyond and behind it. In this, as well as other instances that I have met with, the capsular opacity resembled that siliquose condition which follows upon accidents in which, the capsule being ruptured, the substance of the lens is absorbed, and the two hemispheres, thickened and rendered opaque by inflammatory action, adhere, and present the appearance so well known, not merely to the oculist, but to the general hospital surgeon. In Lynn's case, however, the volume of the lens was preserved, although the opaque capsule, viewed in profile, seemed to project beyond its surface; and finally, after the lens was completely absorbed subsequently by keratonyxis, this opaque body, partially obscuring the pupil, had



to be removed, through an opening in the cornea. The needle makes no impression on such formations; if they are small and central, they start from the surface, as in the case of Mooney, already alluded to; but if they occupy so large a portion of the capsule as in the case of Lynn, the cutting side of the needle should be passed round the edge, so as to bring the lens into contact with the aqueous fluid; and, subsequently, if they cannot be displaced from the field of vision, they should be extracted.

Art. V.—*Two cases of Excision of the Knee-Joint for Forcible Separation of the lower Epiphysis from the Shaft of the Femur.* By Edwin Canton, F.R.C.S., Surgeon to the Charing Cross Hospital, and Lecturer on Surgical Anatomy.

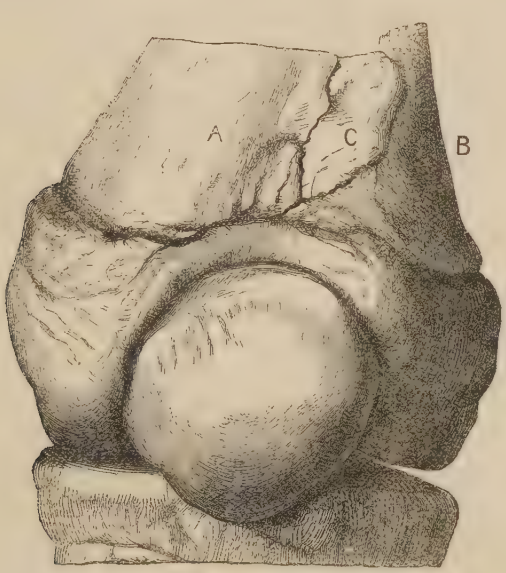
Case I.—William Jarvis, aged 15, was admitted into the Charing Cross Hospital, under my care, October 21, 1859. At the time of the accident he had sustained, he was playing with another lad, and, with the view of eluding him, was about to run under the body of a horse that stood close by. The animal, however, seeing him approach, suddenly rose on his fore-feet, knocked him down, and then kicked him violently, just above the left knee, with one of his hind hoofs. On attempting to rise, the boy found himself unable to stand; and, to escape further injury, he was forced to roll quickly along the ground out of the horse's reach.

On admission, the affected limb was found to be shorter than its fellow; foot quite everted; leg slightly flexed; patella directed outwards; great and general swelling around the knee, and with such distortion of the parts, as to give the impression of the tibia being dislocated backwards, and somewhat outwards. The inner femoral condyle appeared to project unduly, and the skin covering it was tense and abraded. On the outer side, and above the patella, a forward elevation of bone could be felt. By extension and counter-extension, the due length of the limb was restored, the patella resumed its natural position, and the projections referred to became obliterated. The usual appliances maintained adjustment.

No untoward symptom occurred until the end of October, when the incessant restlessness of the patient, from his intolerance of the necessary restraint, gave rise to violent inflammation of the joint. On the inner side of the knee the soft parts

began to slough, and in two days afterwards, the subjacent bone projected through the mortifying tissues. The part protruded was the lower end of the shaft of the femur. The high constitutional disturbance which was set up forbade further efforts to save the limb entire, and I excised the knee-joint, by the usual form of operation, on the 3rd of November.

No peculiarity marked the further progress of the case; and after the lapse of several months, the patient walked out of the hospital, cured.

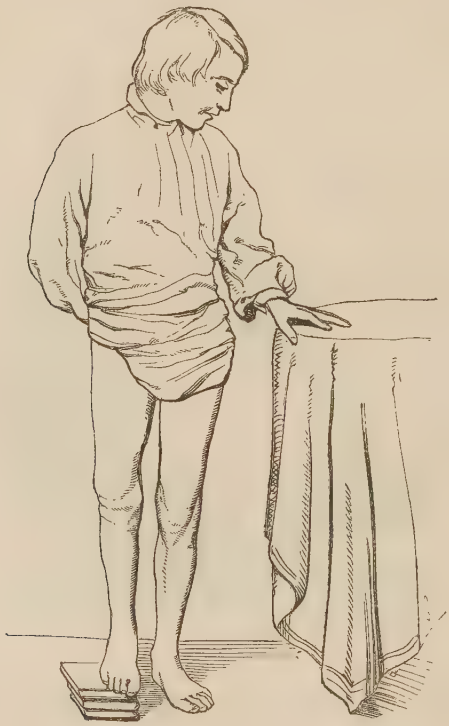


The parts removed show a separation of the shaft of the femur from its lower epiphysis to have occurred in about three-fourths of its circumference (A), while the remaining fourth is still firmly fixed in normal position (B). An oblique fracture extends through the whole thickness of the shaft, and, commencing at its outer side, reaches down to the epiphysis, where it runs into the line of separation between these two parts.

Between the attached (B) and separated (A) portions, a fragment (C) is seen, which, in the engraving, is shown restored to its place; but which had been driven backwards at the time

of the accident, between B and A, completely into the popliteal space, whence I removed it during the operation.

Twelve months after the excision of the joint, I again saw and examined the patient (Nov. 3, 1860). From a photograph then taken of him, the annexed outline has been accurately made.



The tibia and femur were firmly incorporated with one another; the patient experienced no inconvenience, and had walked twelve miles without fatigue.

Case II.—G. C., aged 8 years, was admitted into the Charing Cross Hospital, under my care, having fallen from the back of a cart, where he had been riding; and, in the descent,

his left knee became entangled between the spokes of one of the wheels.

The joint was found to be greatly misshapen, and an external dislocation of the femur was believed to have occurred; so palpable, indeed, did the nature of the injury appear to be, that the house-surgeon at once, and with ease, restored the form of the part by pressure made in two opposite directions, viz., inwards against the femur, and outwards against the tibia. In addition to this injury, there was a large scalp-wound at the back of the head, which had implicated the branches of both occipital arteries; the hemorrhage had been profuse.

The night was passed in great restlessness, though an opiate was administered. The next day, severe pain in the knee was complained of, and which arose from malposition of the fragments consequent on the patient's inquietude. In effecting re-adjustment, the soft parts against which the lower end of the femoral shaft had been driven were seen to be much ecchymosed; and in a few days, a slough having formed there, and begun to separate, the bone protruded, showing at its lower surface that appearance which characterizes the part for attachment to the articular extremity. Various means were had recourse to, with the view of keeping the parts properly *set;* but, in consequence of the severe nature and painful state of the scalp-wound, the boy, besides his general restless state, was continually throwing his body towards the opposite side of the injury, so that his head might recline sideways on the pillow; hence, the femur was proportionately directed outwards, protruding through, and irritating the wound by which it had so lately found egress. Suppuration soon took place, and threatened to become profuse; the constitutional irritation ran high; the appetite had failed; sleep could hardly be procured; and it speedily became evident that, unless some decisive operative measure was instituted, the patient would die.

I excised the knee-joint, making the H incision; the outer cut included in it the opening already described. After the joint-ends of the femur and tibia, together with the patella, had been removed, a very thin slice was sawn from the free end of the thigh-bone, so that more even adaptation with the tibia might be ensured. No attempt at primary union followed. The restlessness of the patient continued almost unabated, and defied every means adopted to quiet the system, and maintain the limb at rest. (Much of the scalp-wound was still unclosed,

and appeared to be the source of irritation.) About an inch of the femur protruded, and could in no manner be kept reduced and covered; and, it becoming evident that this part tended greatly to increase the general *malaise*, at the same time that it was quickly passing into a necrosed condition, I removed it by the saw. The edges of the wound could be now brought once more together, and were so maintained, with an opening preserved at the outer side, so as to allow of free escape for the discharge, which slowly began to assume a more healthy character.

From this time, matters appeared to progress favourably; the patient became calm; sleep and appetite were restored; the wound of scalp and knee, in process of time, healed. No osseous union, however, took place between the femur and tibia —a fibrous one only resulted. To have cut down again to the bones, and removed more of them, considering what I had had to take away, in addition to the loss entailed by the first operation, would have conferred, in its great diminution of length, as useless a limb as the patient now laboured under, and I had no alternative but to amputate at the thigh. Accordingly, I removed the limb, when the boy's health had become thoroughly re-established. He is living, and well.

Observations.—The form of accident, of which the above cases are well-marked examples (*though not even alluded to by many of the highest authorities of the present day*), is, nevertheless, one fraught with great pathological and surgical interest. Valuable illustrations may be obtained therefrom of that improvement in modern surgery, which teaches the preservation of a limb through sacrifice of a joint,—affording, at the same time, a strong contrast to the unnecessary mutilation which formerly characterized the treatment of this injury, or that non-interference which resulted in death.

In an example of this injury, occurring in the practice of Coural[a], amputation was resorted to. The age of the patient was eleven years. Removal of the limb was also adopted in a similar case related by Adams, of Dublin[b].

Sir C. Bell[c] relates the case of a lad, aged 13, who met with this accident. "Suppuration took place in the joint; and by the delirious restlessness of the patient, and ulceration

[a] Archiv. Génér. de Méd., tom. ix., 3eme Serie, p. 267. 1825.

[b] Cyclopœdia of Anatomy and Physiology, Art. "Abnormal Conditions of the Knee-Joint," vol. iii. p. 69.

[c] Observations on Injuries of the Spine and of the Thigh-bone, page 42. London: 1824.

of the integuments, the bone was thrust through. A consultation was necessary; and, on examination, it was found that not a fracture had occurred; but the spongy extremity of the shaft projected, whilst the epiphysis retained its connexion with the joint. The lad died."

Liston[a] has recorded an instance of forcible separation of the lower femoral epiphysis, occurring in a girl, aged fourteen years. Firm union took place very nearly in the same manner as shown in the following case. The knee continued painful and swollen; and there was a halt in walking. After a lapse of about three years, extensive suppuration took place in the lower part of the thigh, and round the knee-joint; and amputation very soon became indispensable for the preservation of life.

Where union of the fragments has eventually taken place, the mischief has not, in certain instances, terminated with their

[a] Elements of Surgery, Second Edition, p. 721, London: 1840

juncture:—e. g. Sir C. Bell[a] describes a case where osseous consolidation occurred; but the limb was obliged to be removed many years afterwards, on account of a popliteal aneurism occasioned by the projecting extremity of the shaft.

Not only may the epiphysis of a bone be separated from its shaft by violence, but it may also become disunited and displaced from disease. An interesting specimen was exhibited at the Pathological Society of Dublin[b], of a separated upper

[a] *Loc. cit.* p. 42.

[b] Dublin Journal of Medical Science, 1839. I am indebted to my colleague, Mr. Barwell, for the subjoined reference, wherein, I believe, we may find the earliest recorded instance of the *surgical* removal of the head of the femur.—"*Coxæ articuli suppuratio cum secessione coxæ femoris solidata.* Anno 1730. Puella rustica, ætat. 14, annorum coxæ articulus tumescit dolet perumpetur chirurgus dilatat foramen naturâ factum, extrahit totum ossis femoris caput. Subjecit posthâc in ulceris cavitatum, *Myrrhæ tincturam porro fluxum Ung. fel. W.*, stringat eam denique arcto vinculo raro deligat, atque sex septimanarum curriculo consolidat, ut puellam postmodum libere liceat manea inseperit." John Daniel Schlishting, M. D. Philosophical Transactions, vol. xlii. p. 274.

epiphysis of the femur, which was spontaneously extruded, in a case of morbus coxarius. Richel[a] describes the particulars of a case, and adds an engraving of the parts, where the lower epiphysis of the thigh-bone, having become detached in the progress of disease of the knee-joint, was completely displaced to the lower and outer side of the shaft of the bone.

It is, of course, only in comparatively young subjects that the form of injury under consideration can occur. "The several epiphyses of the femur," observes Humphrey[b], "are united to the shaft in the inverse order of their appearance: the lesser trochanter, soon after puberty; the great trochanter, at about 17; the head, at 18; and the lower epiphysis, from 20 to 25."

In neither of the cases which fell under my own care, had I the opportunity of seeing the patient immediately on his admission; both were received and attended to by my house-surgeon; and I found the limb, in each instance, arranged in splints at my next morning's visit, when I was informed of the mode of production of the injury, and told that a dislocation of the knee had been the result. Reduction was stated to have been accomplished with facility. Judging from the ages of my patients (15 and 8 years), and considering the particular manner in which violence had been inflicted, I expressed my belief that an error in diagnosis had, not improbably, been made; for, such a degree and form of injury would, with the epiphysis still unconsolidated with the shaft of the bone, more readily produce disjunction of the former, than cause a luxation by rending asunder those numerous and strong ligamentous ties, within and without, which complete and fortify so large and complex a joint as that of the knee[c]. My opinion, never-

[a] Diss. de Epiphy. ab ossium diaphysi diductione, in Sandifort's Thesaurus. Roterd. 1768. Klose has seen, in the space of eight years, thirteen cases of separation of the epiphysis from the shaft, as the result of disease, occurring in young subjects; seven of these were of the femur at the knee. He names the affection, separation of the epiphyses (epiphysentrennung), or, meningo-osteo-phlebitis. Prag Vierteljahrschrift, for January, 1855, p. 97. Stromeyer refers to a similar disease, under the name of ostitis articularis peracuta, or, Arthrophlogosis totalis—*Handbuch der Chirurgie*, vol. i. p. 477.

[b] A Treatise on the Human Skeleton, p. 477,—London: 1858,—a work I cannot too highly commend. "The femur is complete, by the union of its parts, after the twentieth year."—Quain and Sharpey, Elements of Anatomy, Fifth Edition, p. 160. London: 1848. Otto remarks, "I have observed in some men of twenty-two, and in one of twenty-three years, the separation of all the epiphyses; and the same also in the skeleton of a man of twenty-seven years, in the hospital of St. Francis, at Naples."—*Compendium of Human and Comparative Pathology*, translated by South, note 5, p. 127. London: 1831.

[c] "All dislocations of the knee are exceedingly rare, yet we occasionally read of them; and, perhaps, in the course of twenty years, there may be one case brought into a large hospital."—S. Cooper, First Lines of Surgery, Seventh Edition, p. 716. London: 1840.

theless, was, at the time, deemed of little weight, inasmuch as I had not been present to examine either of the patients on his admission; and the symptoms of dislocation had appeared to be so obvious, that the supposed reduction was, at once, and with ease, effected. The issue of the cases, however, sufficiently justified the view I had ventured to entertain, and which had its origin in the above considerations.

In regard to the upper epiphysis of the femur, Roche and Sanson[a] have remarked:—"Chez les jeunes sujets, les causes capables de produire une fracture de col du fémur ne déterminent qu'un décollement de l'épiphyse."

With respect to the mode of production of the injury, in three instances (one of Sir C. Bell, that of Liston, and Case II.), the leg, or knee, became entangled and twisted between the spokes of a wheel in motion. In two other cases (by Sir C. Bell, and Coural), the limb was fixed to above the knee-joint; whilst the body was thrown forwards, or turned round. In Case I. violence was immediately applied just above the articulation; and in Adams' case, as well as in one figured by Maclise[b], the manner in which the force was directed is not stated.

It has been shown that union by bone may take place after this injury; nevertheless, in two instances where this was the result, subsequent disease, and which might be said to date from the original mischief, necessitated, eventually, the removal of the limb. In the other cases already mentioned, amputation was more speedily required, in consequence of the violent articular inflammation which sprang up with profuse suppuration, to endanger the lives of the patients through the high constitutional disturbance so hastily supervening. Lastly, Case I. has proved with what success excision of the knee-joint may supersede removal of the limb for this accident; and I believe it is not unreasonable to presume that in Case II., the result would have been equally favourable, had the reparative powers of the patient been more adequate to the work of repair; and great, indeed, is the amount of constitutional vigour which the surgeon has to expect before union and consolidation of all the textures he severs can be fully accomplished. In place of a series of destructive processes rapidly advancing, and from a joint radiating, as it were, with still increasing in-

[a] Nouveaux Elém. de Path. Méd-Chirurg., Third Edition, vol. ii. p. 191. Bruxelles. 1834.

[b] On Fractures and Dislocations, Plate xxxi. Fig. 5. The separation of the epiphysis is here accompanied by a transverse fracture of the shaft of the femur in its lower-third.

tensity to the implication of the whole system, the surgeon, by his performance of excision substitutes, so to speak, a condition the equivalent of a severe *compound fracture;* and in bringing the divided parts at once together, follows out a good old surgical maxim, which inculcates the endeavour to render that fracture, as speedily as possible, a *simple* one; when, by attention to position, maintenance of quietude, and removal of discharge, he has *now*[a] every reason to anticipate that his measures may not only preserve a threatened life, but retain, also, a useful limb.

Art. VI.—*The Writ "de Lunatico Inquirendo" in the Case of Jonathan Swift, D. D., Dean of St. Patrick's; with Observations.* By J. T. Banks, M. D., M. R. I. A., King's Professor of the Practice of Medicine; Visiting Physician to the Richmond Lunatic Asylum, &c.. &c.

Chance lately placed in my hands the legal document which proves that, after due inquiry before commissioners appointed under the great seal, Jonathan Swift was declared to be "a person of unsound mind and memory," and placed under the protection of the Court of Chancery. The original, with the autographs of the commissioners and the jury, I had the honour to bring under the notice of the Royal Irish Academy, at a recent meeting of that learned body.

So much of doubt and uncertainty rests upon many passages in the life of Swift, that it is of great importance to add any fact, the authenticity of which is beyond all question. As a proof of the contradictory nature of some of the statements, I may mention that the mode of his death is still an unsettled point.

Lord Orrery says, "His death was easy, without the least pang or convulsion; even the rattling in his throat was scarce sufficient to give any alarm to his attendants, till some very little time before he expired." Falkner, on the contrary, asserts that "he died in very great agony; having been in strong convulsive fits for 36 hours before." Every circumstance, every event, in the life of the illustrious Dean of St. Patrick's must be fraught with interest, connected as they are with one who

[a] "Of 184 cases in all, collected by Butcher and Price, the mortality amounts to 21·2 per cent., which is less than amputation of the thigh; and, in nearly 50 per cent., the result is said to have been a useful limb."—Pirrie, The Principles and Practice of Surgery, Second Edition, p. 736. London: 1860.

was not only distinguished by the excellence of his genius, but also by the unswerving character of his patriotism.

To all who are familiar with the history of the life and labours of Swift, it is well known that many years of his life were clouded by the threatenings of impending disease; and that he himself was painfully conscious that the symptoms under which he laboured were frequently the precursors of mental aberration. The psychological physician will easily recognise, by a study of Swift's eventful history, causes which his experience informs him are potent in the production of mental alienation. The storms to which he was exposed were well calculated to prostrate a fabric which rests on so insecure and unstable a basis as the human mind. Poverty and dependence in youth, so galling to a haughty spirit such as his, disappointed hopes, blighted affections, the loss of those most cherished, with many other circumstances, combined to produce the disease, the anticipation of which had so long, like an evil spirit, haunted him, and well nigh embittered every moment of his existence.

That cerebral disease existed, and can be traced back to an early period of his life, is beyond all doubt. Few and meagre as the records are which we possess on the subject of Swift's disease, and solely derived from himself, and others even less likely to be well informed, we can have no difficulty in arriving at this conclusion.

It does not appear, as far as we have the means of ascertaining, that he inherited any predisposition to psychical disease; but we are not in possession of any accurate information regarding the mental health of his progenitors.

We know that his grandfather, who distinguished himself by his zeal and sufferings in the Royal cause during the "great rebellion," died at a good old age; and we do not hear of his having been afflicted with any mental infirmity. His father died young, before his son—who was destined to play so conspicuous a part in the busy world of politics and literature—had seen the light. It may be supposed that from his mother Swift inherited much of that extraordinary humour for which he was so remarkable. If this be so, as there is reason to believe, it is only in accordance with a fact often noticed, namely, that talent is more frequently transmitted from the mother than the father.

That the offspring not only resembles the parent in outward form and feature, but also frequently in mental endowments, we have ample proof from daily observation. No fact is more familiar to the physician, than the transmission of disease, psychical as well as physical; and the same may be affirmed

with regard to mental peculiarities. This, I believe, may be looked upon as a fact ascertained by reiterated observation; nevertheless, Mr Buckle, in his History of Civilization, has attempted to shake our well-founded faith in the transmission of hereditary talents, hereditary vices, and even of "hereditary madness." If the distinguished and eloquent author had taken the trouble of consulting the records of hospitals for the insane, or the works on mental disease, he would find himself compelled to give up his idea regarding the non-hereditary nature of mental maladies.

M. Moreau, Physician to the Bicêtre, one of the most accurate observers with whom I have ever come in contact, strongly advocates the doctrine of the transmission of psychical disease in his learned and deeply interesting work, "La Psychologie morbide dans ses Rapports avec la Philosophie de l'Histoire ou de l'Influence des Neuropathies sur le Dynamisme Intellectuel." He says, "L'hérédité est la source des neuf-dixièmes, peut-être, des maladies mentales." In this opinion of the hereditary nature of insanity, he only agrees with all who have studied the subject by the light of experience—Esquirol, and other French alienist physicians; and the most distinguished English psychologists, as Conolly, Bucknill, Winslow, and others.

Swift is included by Moreau in his group of men who presented symptoms of "insanity, properly so called;" and in goodly company, doubtless, is he placed; but probably not one among these aristocrats of talent was possessed of a more original mind. M. Moreau considers that diseased states of the brain powerfully favour the development of the highest intellectual functions: it would appear, according to him, that in point of fact the most exalted genius and insanity are cognate conditions. He evidently agrees in opinion with the ancient sage who says, "Nullum magnum ingenium sine mixtura dementiæ." Swift himself said, "Madness is the source of all human genius."

The subject is one of surpassing interest; and it must be conceded to M. Moreau, even by those who differ widely from his conclusions, that he has handled it with much ingenuity and ability.

It is not my present purpose to enter minutely into the subject of Swift's case, from a medical point of view; that has already been done by my friend Dr. Wilde, in the pages of this Journal, and well has he performed the difficult task which he assigned to himself, and that too in the absence of any record from the many physicians who ministered to the Dean.

When we remember how eminent some of the physicians

were who endeavoured to alleviate Swift's sufferings, it is a subject of much regret that they have not bequeathed to posterity one line on the subject of the disease of their illustrious patient.

Among his physicians were Sir Patrick Dun—the benevolent founder, like himself, of an hospital which bears his honoured name—and Dr. Arbuthnot, physician to Queen Anne, of both of whom he speaks in terms of affection Of Arbuthnot he says, "He was the only man of the faculty who seemed to understand my case; but could not remedy it." Writing to Pope, he says, "The death of the Doctor (Arbuthnot) has been a terrible wound to my heart." Swift had begun to feel acutely the loss of friends, which Sir William Temple said is a "tax on long life."

Numerous as the Biographers of Swift have been, and carefully as they have sought for information from every available source, there is one passage in his life, and that the most melancholy, with which they appear to be unacquainted,—I allude to the fact of his having been made a "Chancery Lunatic." It is true, Sir Walter Scott alludes to persons having been appointed to take care of him. He says, "His estate was put under the management of trustees, and his person confided to the care of Dr. Lyons, a respectable clergyman."

Dr. Delany says, "In the beginning of the year 1741, his understanding was so much impaired, and his passion so greatly increased, that he was utterly incapable of conversation; strangers were not permitted to approach him, and his friends found it necessary to have guardians appointed of his person and estate." We shall see, by referring to the legal instrument, how incorrect Delany is as to the date. It was not until the 17th of August that the writ "de lunatico inquirendo" was issued; and it may be remarked, that it was returned on the 19th of the same month, although the return was not required before the 3rd of November. It is evident, then, that the biographers knew nothing of the writ, or this inaccuracy could not have occurred. It is strange, from this statement of Delany's, and from other proofs of the utter prostration of Swift's intellect, at a period long antecedent, that the jury only found him to be of unsound mind for three months prior to the date of the Commission.

That we have no record of the examination of medical men or "experts," on an inquisition which was to decide upon the mental soundness or unsoundness of a man who "did the highest honour to his country by his parts, and was a great blessing to it by the vigilance and activity of his public spirit," is matter of regret. An inquisition upon such a man

must have excited much interest, and the more strange that all the facts connected with it should not be a matter of public notoriety. But we do not find that even the long and laborious research of Mr. Weld Hartstonge obtained for his friend, Sir Walter Scott, any information on this point. It appears, also, that Scott obtained materials for his life of Swift from the Rev. Mr. Berwick, "so well known to the literary world," and from other sources likely to be in possession of authentic information.

I may mention, that Judge Berwick, son of the Rev. Mr. Berwick, to whom Sir Walter Scott expresses his obligations, is the possessor of a portrait of the Dean, which is probably the best which has ever been painted.

The petitioners for the writ de lunatico inquirendo were the Rev. John Grattan and the Rev. James King, both of whom were executors of the will, and to each the Dean made a bequest.

"I bequeath to Mr. John Grattan, Prebendary of Clonmethan, my silver box in which the freedom of the City of Cork was presented; in which I desire the said John to keep the tobacco he usually cheweth, called pigtail." "I bequeath to the Rev. Mr. James King, Prebendary of Tipper, my large gilded medal of King Charles the First, and on the reverse a crown of martyrdom, with other devices."

Of the Commissioners to whom the writ was directed, the first on the list is the Right Hon. Luke Gardiner.

Dr. William Cooper was Chief Examiner of the Court of Chancery.

Charles Grattan is marked on the list of Swift's friends with the letter G. All his friends were classed into grateful, ungrateful, indifferent, and doubtful.

Alderman Perceval Hunt was Sheriff of Dublin in 1718, and Lord Mayor in 1755. Alexander M'Auley was one of the executors of the will; he was Judge of the Consistory Court, and one of the first Governors of Swift's Hospital. By the will we learn that M'Auley was one of Swift's most esteemed friends. He is thus mentioned:—"To Alexander M'Auley I bequeath the gold box in which the freedom of the City of Dublin was presented to me, as a testimony of the esteem and love I have for him, on account of his great learning, fine natural parts, unaffected piety and benevolence, and his truly honourable zeal in defence of the legal rights of the clergy in opposition to their unprovoked oppressors."

Alderman Macarel was Lord Mayor in 1738, and Alderman Pearson was Lord Mayor, 1730.

John Rochfort was one of the witnesses to the will.

Sir James Somerville was Sheriff in 1720, Lord Mayor in 1736, and at the same period was knighted. This gentleman was subsequently created a Baronet, by patent, dated June 14, 1748.

Eaton Stannard was Prime Sergeant at Law, and Recorder of Dublin; he was one of the executors of the will.

Philip Tisdall, Judge of the Prerogative Court ; Whitney Boleyn, King's Counsel.

Some of the Commissioners, it may be noticed, were Swift's intimate friends, from the circumstance of his having selected them as executors to his will, or having remembered them in that document.

The writ was directed to seventeen gentlemen named, as Commissioners; but only seven names are annexed to the inquisition. The writ commanded any three or more of them to diligently inquire into the case.

The jury, upon which devolved such an important duty as determining the mental condition of Jonathan Swift, was strangely constituted. There are some respectable citizens, and probably, from their position in life, intelligent men; but there are also two carpenters and a currier associated with them. Their task, however, was an easy one, under the guidance of the Commissioners; for, from all we can learn, we may fairly come to the conclusion, that long before the time of the issuing of the writ, the mighty mind of Swift had completely broken down. The first faint clouds which foreshadowed the coming event may easily be traced to a comparatively early period in the life of this truly great man; but the storm which was destined to utterly overwhelm the shattered bark, had already broken over it.

The life of Swift is still to be written; and I have reason to believe it is in progress, and, moreover, that it is in good hands. My object in publishing the contents of the important instrument which I had the good fortune to discover, is to fill up what has hitherto been a blank in the darkest page of the history of the renowned Dean of St. Patrick's.

"George the Second, by the grace of God, of Great Britain, France, and Ireland, King, Defender of the Faith, and soforth, to our trusty and well-beloved the Right Hon. Luke Gardner, Esq.; Eaton Stannard, Recorder of the City of Dublin, Esq.; Philip Tisdall, and Boleyn Whitney, Esqrs.; Doctor William Cooper, and Doctor Thomas Trotter; Sir James Somerville, Alderman John Macarell, Alderman Percival Hunt, Alderman Nathaniel Pearson, Alderman Robert

King, Thomas Le Hunt, and Alexander M'Auley, Esq., William Harward and John Rochfort, Esqrs., Charles Grattan and Bellingham Boyle, Esqrs., greeting: Whereas it is given us to understand, by the petition of the Rev. John Grattan and the Rev. James King, that the Rev. Doctor Jonathan Swift, Dean of St. Patrick's, Dublin, hath, for these nine months past, been gradually failing in his memory and understanding, and of such unsound mind and memory that he is incapable of transacting any business, or managing, conducting, or taking care either of his estate or person: We, being willing to provide a remedy in this behalf, do command you, or any three or more of you, that you repair to the said Doctor Jonathan Swift, and, by all proper ways and means, you examine him; and, moreover, by the oaths of good and lawful men, by whom the truth of the matter may be best known, you diligently inquire whether the said Doctor Jonathan Swift be a person of unsound mind and memory, and not capable of taking care of his person or fortune, as aforesaid; and if he be, how long he hath been so, and of what lands and tenements, goods and chattles, the said Doctor Jonathan Swift was possessed of at the time he so became of unsound mind and memory, or at any time since, and what is the yearly value thereof, and who is his next heir; and such inquisition as shall be then found, you, or any three or more of you, shall, openly and distinctly, make return thereof to us, in our Chancery in Ireland, on the third day of November next, under your seals and the seals of those by whom the said inquisition shall be made, together with this writ.

" Witness our Justices General and General Gouvernours of our said Kingdom of Ireland, at Dublin, the Twelfth day of August, in the Sixteenth Year of our Reign.

"Domvile.

" Exd. Ed. Madden,
Deputy Clerk of the Crown and Hanaper."

" The executors of the within commission appear by the inquisition hereunto annexed—

Luke Gardiner,
Philip Tisdall,
Henry Rochfort,
William Harward,
Bellingham Boyle,
Percival Hunt,
John Macarell.

"An inquisition taken before us, the Right Hon. Luke Gardiner, Philip Tisdall, Esq.; John Macarell, Percival Hunt, Alderman William Harward, John Rochfort, and Bellingham Boyle, Esqrs., Commissioners by virtue of a Commission of our Sovereign Lord George the Second of Great Britain, France, and Ireland, King, Defender of the Faith, and soforth, Bearing Date the Twelfth Day of August, in the Sixteenth year of the Reign of his said Majesty, at the Deanery House of St. Patrick's, Dublin, the seventeenth day of August, in the year of our Lord one thousand seven hundred and forty-two.

"The Names of the Jury of honest and lawfull Men sworn to enquire and examin of and into the matter specified in said Comission.

Edwd. Hunt, Alderm^{n}.	John Cooke, Hosier.
John Adamson, Merch^{t}.	John Cumin, Carpenter.
Robt. Donovan, Merch^{t}.	Erasmus Cope, Jeweler.
Arthur Lamprey, Chand^{r}.	John Sican, Merch^{t}.
Thoms. Hamilton, Brewer.	John Martin, Currier.
John Walsh, Carpenter.	Joshua Barrington, Merch^{t}.

"Gentlemen,

"Your Issue is to try and Inquire Whether the Rev^{d} Doctor Jonathan Swift in the annexed Comission named be a person of unsound mind and memory, and not capable of takeing care of his person or Fortune; and if he be, how long he hath been so, and of what Lands, Tenem^{ts}, goods, and Chattles the said Doctor Jonathan Swift was possessed off at the time he so became of unsound mind and memory, or at any time since; and what is the yearly value thereof, and who is his next Heir."

"Wee Find that the Rev^{d} Doctor Jonathan Swift in the annexed Comission named is a person of unsound mind and memory, and not capable of takeing care of his person or Fortune, and that he hath been soe since the twentyeth day of May last past. And Wee Further find that the said Jonathan Swift was on the said twentyeth day of May, and still is seized and possessed of Lands, Thytes, and Tenements of the clear yearly value of eight hundred pounds sterling, and also possessed of goods and Chattles to the value of ten thousand pounds sterling; and It does not appear to us who is his next Heir."

(Here follow the signatures and seals of the twelve "honest and lawfull men" whose names have been recited.)

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

A Book about Doctors. By J. Cordy Jeaffreson, Author of "Novels and Novelists," &c., in two volumes. London, Hurst & Blackett. 1860. 8vo, pp. 628.

"A Book about Doctors." What a grand theme! ennobling to the very writer thereof, the history of philanthropy, benevolence, and natural science—the history of the great and good men who, in all times and climes, have alleviated human suffering, cheered the desponding, comforted the afflicted, poured out the oil and wine on the way-worn and the wounded; the story of the brave and gentle; of those whom the Scripture desires us to honour, who have more than all others carried to their fellows the message of Christianity—"Peace on earth, good will towards men;" and who can, above all others, be tried by that great test, "I was sick, and ye visited me"—the record of the men and the art which has brought strength to the weak, sight to the blind, ease to the tortured, sleep to the weary, and sense to the demoniac—many of whom have died for their brethren, and several of whom have bequeathed their fortunes to the poor. Verily, a glorious subject; let us see how it has been handled by Mr. J. Cordy Jeaffreson. Here is "The Book about Doctors," in two volumes, octavo, bound in gold and morone, with a frontispiece to the first, copied from Hogarth's caricature of the old broad-wigged doctors, smelling their vinaigrette-canes, and tasting the contents of a male urinal! And who are the doctors? Mesmer, the impostor; Roger Grant, the tinker; Valentine Greatrakes, the stroking doctor, whose "cures" we gave an account of in vol. iv. p. 254, of this Journal; Loutherburgh and his wife, who humbugged the public into the belief that *their* faith had made them whole; Townsend; St. John Long; Morrison, the pill manufacturer; Graham; and a host of other equally notorious quacks, from the days of Merry Andrew, and Katterfelto the

conjurer and mountebank, down to the present time. If the work had been labelled "A Book about Quacks, by a Humbug," the title would not have been inappropriate. And yet this is a book which a London weekly contemporary styles "a compliment to the medical profession, and an acquisition to its members; a book to be read and re-read; fit for the study and the consulting-room, as well as the drawing-room table and the circulating library." No wonder that another London contemporary, celebrated for its sneers at medicine, should say, on reviewing such a work as Mr. Jeaffreson's, "There is no denying the fact that, though medicine does hold its place as one of the *learned* professions, it does not do so without a struggle." And again, "The ministering to the wants and diseases of the body is by itself a gross and menial office." England is *par excellence* the land of quacks, and therefore we can understand why a gentleman of "the Rolls Chambers, Chancery Lane," who has so many opportunities of studying the character of the quack and mountebank, should be an authority upon such subjects as this so-called "Book about Doctors" treats of.

To make up and make out a book about doctors, we have a few biographies of well-known celebrities, such as Radcliffe, Mead, Arbuthnot, Blackmore, Cooper, Abernethy, &c., injudiciously abridged from well-known sources, interlarded with a few hackneyed professional jokes, and stale pointless anecdotes, so tamely told, that one is inclined to think the author does not see the wit of them himself. A good example of this is given in the recital of Curran's interview with Abernethy, in which Mr. Jeaffreson makes the ex-Master of the Rolls introduce himself to his countryman, by saying, "My name is John Philpot Curran," whereas the gist of the story is that Abernethy made a diagnosis of the man before the latter had time to announce his name. Will our readers forgive us, if we relate the anecdote as we had it more than once, twenty years ago, from the lips of dear old Cliff, in the Museum of the London College of Surgeons :—

When Curran left Ireland, and took up his residence in London, before the days of Clubs and Athenæums, he suffered from *ennui* and dyspepsia, the natural consequence of want of friends and occupation, but more particularly of that choice and witty society of which he himself was the ornament, from the time of the 'Monks of the Screw' to the last days of his residence at Rathfarnham. Like all the dyspeptic world of the day, he sought the aid of Abernethy, and, lawyer-like, wished to state his case; but as such harangues were not permitted in

Abernethy's summary court of medicine, the great orator was shut up with the brusque "Yes, I know; it is all your stomach. Here's a prescription; take one of these blue pills; eat my biscuits; read and attend to the directions in this little book; and call the day after to-morrow." In vain the lawyer essayed to speak; he was marched to the door, where another patient was in readiness to occupy his place.

The scene was repeated in a few days; the doctor looked at the patient's tongue, and recommended a continuation of the treatment. Upon the third visit (for story-mongers, as well as fairy-tale relaters, must have a climax, and three is a mystical number), Abernethy was writing a letter, when the patient was shown in; and, startled by hearing the door-key turned in the lock, he saw, on looking up, the yellow, monkey-faced, but bright-eyed Irishman, whose case he would not listen to, deliberately putting it in his pocket. His first impression was that the man was a lunatic, and so he measured his patient, and looked to the poker. On the first exclamation, Curran, who was a most consummate mimic, assuming the tone and look of Abernethy, said, "Sit down; show me your tongue; it's all your stomach; I know all about it." So astonished was the great physician, that he motioned his eccentric patient to a seat, who, resuming his natural voice, said, "Now, sir, I have been here twice, paid you your fees, followed your directions, taken your pills, eaten your biscuits, and read your trashy book; and I am not one bit bitter; and the reason is, that you have never listened to the history of my symptoms." On this, Abernethy lay back in his chair, folded his hands resignedly, and said, "Well, sir, go on; begin at the beginning; and give an account of your mother and grandmother, and relate the family history of your whole seed, breed, parentage, and education." The patient, assuming the attitude, and imitating the nasal twang of the street-criers of last speeches and dying declarations, drawled out, "I was born of poor, but honest parents, in the town of Ballyragget;" and thus proceeded, until he arrived at, "and my name is——," when Abernethy, starting up, exclaimed, "John Philpot Curran!" "Yes," said the patient. "Then, why the devil did you not tell me so at once? You are not a bit sick, my dear Curran; don't take any more physic, but come and dine with me to-day." Cliff told the writer of this that he was present at the dinner, and saw the scene rehearsed by the pair of worthies. Abernethy and Curran became, as it is known, fast friends.

The anecdote has a moral: the patient must be heard; and

the less real disease, the more graphic and lengthened the details. It must be a great comfort to a patient to unburthen himself of the pent-up imaginings of his disease, which he has rehearsed again and again before he came to get a guinea's worth of listening, and a prescription. We remember a fellow-student at a German university, who, we were happy to hear, some years after we had met, was rapidly acquiring a lucrative practice. On asking a friend how this good luck had come about, the answer we received was, "He listens, be the story ever so long—he never interrupts—that is the great secret of his success."

Some years ago, an old lady, who had been for some time a patient of the writer's, for a very simple and ostensible disease, waited upon him, and said—"Now, my dear doctor, I know your time is precious, so here are two guineas, and you must listen to what I have to say." The good lady detailed her symptoms, real and imaginary, then appealed to her maid as to whether she had forgotten any, and finally producing a long list from her pocket, repeated her case: and, rising to go, thanked the surgeon for his extreme kindness, and said she had no doubt that she would derive great benefit from his advice, although he had never opened his lips, nor written a line. Some people might think the money hardly earned.

We remember attending an old judge, a most worthy and learned man, but somewhat pragmatical and pedantic. There were four of us in attendance; after assembling in the drawing-room daily, we were ushered in a body to the sick chamber, where there was little light, and the bed-curtains were closely drawn. The patient having been assured of our presence, then opened the pleadings, stated the case as it occurred since the last visit, examined the various symptoms as witnesses, charged the jury, admitted the light, and then demanded our verdict.

Lest we might fall into the story-telling vein, we must get back to the subject of our review. It is true that the author has reprinted the exposition of all the quacks he mentions; but the charlatans are so mixed up with the true men, that the general reader, and the public, must look on all as part of the "fallacies of the faculty;" and, even when making short extracts from Pettigrew, and other writers, it is not always the bright side of the man that is presented to the public palate. Thus we read of the "parsimonious temper and habit" of Sir Hans Sloane; the extreme dirt and slovenliness of Humphrey Davy; the rudeness of John Hunter; and the drunkenness of Friend, of whom, by the way, Mr. Jeaffreson tells the story of

"Dead drunk, by G——," when feeling a titled lady's pulse, —which, in this country, we are in the habit of attributing to Harvey.

A Book about Doctors is much wanting; but not such a work as that under review, which, such as it is, is almost entirely about Englishmen—the Scotch and Irish medical writers are ignored. No—not altogether; the Dublin men are represented, not by Molyneux, Mosse, Dun, or Cheyne, or Graves, or Colles, or Crampton, but by —— who, gentle reader, do you think? Brennan— jolly, drunken, satirical, pasquinading, wrestling, turpentine Brennan—one of whose worst poems, that on the "Dublin Doctors," is quoted; but the fun of which cannot be felt, nor the sarcasm understood, by many, even of ourselves, in the present day. But, although the readers of Mr. Jeaffreson's book may not understand all the allusions to Doctor Big Paw, and "sweet Breeny Creepmouse, O," this quotation from the Broadstone slang they must have specially relished:—

> "Here ends my song on Doctors, O,
> Who, when all damn'd
> In hell are cramm'd,
> Will beggar all the Proctors, O."

Diseases of the Heart; their Pathology, Diagnosis, and Treatment. By W. O. MARKHAM, M. D., F. R. C. P., &c., &c. Second Edition. London: Churchill. 1860. 12mo., pp. 276.

THIS is a second edition of Dr. Markham's treatise, and is, in many respects, a great improvement on the first: much of the introductory matter has been omitted or curtailed, and the whole work has been carefully revised.

The exposition given by Dr. Markham of the symptoms and physical signs of the diseases of the heart is clear, concise, and comprehensive, and renders his work one well adapted as a text-book for the student who is entering on the study of these diseases at the bed-side; and even practitioners, and the readers of the larger books, would often find the short physiological and rational explanations of the principles of diagnosis here given a great assistance. Some books are *hard* for the student, as has been aptly remarked by Dr. Latham, from the very abundance of their matter, and especially if they are at all controversial in their style. To the student such books are often as obstructive, as is the loquacity of many guides and cicerones to tourists. To clinical students, says Dr. Latham, there is no greater im-

pediment of knowledge than over-teaching: the teaching which they most require is suggestive, they have the realities themselves to learn from, the original *book* to read, upon which all sound instruction is but a commentary. Therefore the commentator should only interpose when and where he is needed, and not after the manner of certain critics, who must *help* us with their annotations, where the meaning of the author is clear beyond dispute. Now, Dr. Markham has avoided the error deprecated by Dr. Latham, and has equally steered clear of the other error of obscure brevity; therefore it is that, though we scarcely find a new fact added to the clinical history of cardiac diseases, we recommend the work as a valuable text-book for clinical study. But, valuable as we believe it capable of being in this respect, we think that to the practitioner who may not have very extended opportunities for acquiring experience in the treatment of these diseases it is likely to be still more valuable, from the application he will here find of the doctrines established by the recent advances of pathologic and therapeutic science. This is the most important feature of the work, and the one in which most novelty will be found.

The whole science of medicine has of late been undergoing a process of fermentation, required, our author says, for its purification,—for the removal of the clogs and burthens of errors and crudities which have gathered around it. When we compare the pathology of to-day with the pathology of the past—the diagnosis of to-day with that of the past—the clinical study of disease, as taught to-day, and taught a generation ago —we need not feel any surprise that the practice of medicine has been absolutely revolutionized. The translator of Skoda, and a disciple in the school of philosophical scepticism, it will not be wondered at that Dr. Markham questions the power, or rather asserts the powerlessness of certain agencies, as, for instance, venesection and mercury, in arresting inflammatory processes; and that, while he denies that disease can be evacuated at the mouth of a vein, and that inflammations can be jugulated or strangled in their birth by venesection, he attempts to show the true use of bleeding and of mercury in treatment. Speaking of the treatment of pericarditis by venesection, advocated by Bouillaud and others, he says:—

"A better pathology and a truer method of observation have forced upon the physician, during the last few years, a conviction of the inutility, and sometimes injurious effects of large venesections upon the progress of internal inflammations; and even they, happily few, who still follow the practice of former days, in the treatment of acute internal inflammations, admit that it is only

during the very early periods of the pericardial inflammation, and in patients of strong and robust constitutions, that venesection is of service. That a moderate bleeding may be practised with impunity, *under such conditions,* there is little doubt; and that it often is of much benefit in giving temporary relief to the sufferings of the patient, and in relieving the congestion of the heart and lungs, is certainly true; but that such a bleeding has *any other beneficial effect, or any direct influence over the progress of the inflammation, has yet to be proved.*

"In considering the question of the value of venesection in pericarditis, we must remember:—That every inflammation is a disease of weakness, a condition which, in reference to health, is an asthenic condition; That heat, redness, and increased vascularity are no more signs of a sthenic circulation, than is the hyperæsthesia of a paralyzed limb a sign of increased vigour of its nerve force; That rheumatic pericarditis occurs exceptionally only in persons of robust constitution; That an organ is involved in this inflammation, the constant performance of whose functions is indispensable to life; And that one of the most immediate effects of the inflammation and its products, is to induce a paralysed condition of the muscular structure of the heart; That in pericarditis, the reaction of depression consequent upon the excitement, is great, and sets in early; and that it is in those cases in which the inflammation appears most violent at the onset, that we are most cautiously to watch for and to expect the greatest amount of subsequent depression; That bleeding, moreover, will not arrest the exudation, but, on the contrary, in certain states of the body, will hasten and increase the amount of it; and that, inasmuch as endocarditis is very commonly associated with the pericarditis, bleeding, by promoting the tendency in the blood to the deposition of its fibrinous particles, will increase the danger of permanent injury to which the valves are exposed, through the deposition of fibrine upon them; And that venesection, as we well know, increases the amount of fibrine in the blood, and diminishes that of the red globules—the increase of the one and the diminution of the other being both indicative of weakness.

"Another special danger has been strongly pointed out by Dr. Todd (*Renal Diseases*, p. 412). He says:—

"'An active antiphlogistic treatment creates asthenia; asthenia gives to both rheumatic fever and gout what I may call *the shifting character*, which in both diseases is most perilous. When you find this shifting tendency, depend upon it that the asthenic condition of the patient is that which demands your earliest attention.' 'In this case of acute rheumatism,' he says again at p. 12, 'the loss of large quantities of blood from hæmaturia at an early period of the disease, has not sufficed to keep off a severe attack of pericarditis, . . . nor has it saved the patient from swollen and exquisitely painful joints. On the contrary, the articular, as well as the cardiac symptoms, have been much less tractable than usual.'

"Experience has also shown us, that venesection has no *directly*

beneficial influence over pericarditis; and that large bleedings are prejudicial, and therefore inadmissible in this disease. Nevertheless, that small bleedings are often of very great service *in relieving the congestions of the heart and lungs*, which so often arise as consequences of, and coincidently with the pericarditis, is, I think, an undoubted fact. I have elsewhere spoken of this particular mode of action of venesection; and if the conclusions there arrived at are, as I firmly believe, correct, it necessarily follows—that moderate venesection, practised for the object indicated, viz. to relieve the congestion of the heart and lungs, may be often resorted to with great benefit to the patient.

"In all stages of diseases of the heart in which congestion of the organ occurs, such venesection—duly adapted to the individual case—is of great service. I consider that I have even seen life preserved by timely abstraction of blood in cases of chronic valvular diseases of the heart, where the organ was so overwhelmed and labouring, as to render death imminent."

The paper read by Dr. Markham, before the Medico-Chirurgical Society, in 1858, at the time of the "blood-letting controversy," will, probably, be in the recollection of most of our readers. In it he propounded the doctrine here advocated, that the benefit to be derived from venesection, in cases of internal inflammation, arose solely from the relief thereby afforded to the congestion of the heart produced by the disease; and he gave cases illustrating the benefit arising in this way in pneumonia, inflammation of the viscera of the abdomen, &c. The local abstraction of blood, on the other hand, he asserts to be serviceable, by its effects in emptying the vessels of the inflamed part, and, consequently, the practice is stated to be beneficial only where the blood can be taken from a part having a vascular connection with the inflamed organ. In the case of the pericardium and pleura, this is maintained with the walls of the chest by the internal mammary artery, and hence the well-established benefit derived from leeching in pericarditis; but in cases of endocarditis, the application of leeches to the chest is not, Dr. Markham believes, beneficial. In the main we believe these principles to be true; but it is too well established to be doubted, that leeching over inflamed viscera which have no vascular connexion with the surface is often beneficial, on the principle, probably, of revulsion or derivation.

Mercury, in acute pericarditis, our author believes to be only serviceable when used to regulate the secretions. The induction of its specific effects, once considered essential, is now, he says, generally abandoned: Dr. Taylor's observations first demonstrated its inutility, and subsequent experience has

fully confirmed his opinion. The practice never seems to be much adopted on the Continent, and in Germany it is untried and unknown. Gendrin's opinion, that mercury is completely useless in pericarditis, is, Dr. Markham says, the general opinion of his countrymen.

The salts of potash are proved, by experience, to be most beneficial in the treatment of acute rheumatism; and the author confirms the statements of Dr. Garrod, that they not only shorten the duration of the articular affection, but also lessen the liability of the heart to become affected; and when the heart has been affected before the treatment has been commenced, the salts of potash check the progress of the disease, and prevent the terrible mischief which, when uncontrolled, it so frequently produces. The use of opium to relieve the pain, that is sometimes very severe, is also warmly advocated; but care must be taken not to lock up the secretions, and it is recommended to combine it with small doses of compound colocynth pill. Dr. Markham sums up the general treatment of acute pericarditis in the following extract, with which we must close our notice of this valuable treatise:—

"The *general* treatment, then, of rheumatic pericarditis is in the main—at least during its first periods—the treatment of acute rheumatism. In the commencement of the inflammation, the *local abstraction* of blood, proportioned to the degree of pericardial pain and the strength of the patient, is of service. Should there be signs of co-existing pulmonary and cardiac congestions, small venesections should be resorted to for their relief. Warm fomentations or poultices should be constantly applied over the præcordial region—a method of bringing warmth to the inflamed part, and of giving relief to the patient, which is worthy of more attention than is generally given to it in this country. The beneficial effects of blisters have been doubted by some observers; but I have so often seen them followed by mitigation of the local pain, and apparently also by diminution of the pericardial effusion, that I do not hesitate to use them, when the acute periods of the inflammation have passed away. They do not prevent the application of poultices or fomentations. Dr. Todd advises their use, even in the acutest stages of the inflammation.

"The secretions of the body should be duly attended to during the whole course of the pericarditis; for there is, we must consider, a poison to be eliminated from the system. The bowels should be regularly relieved; and the action of the kidneys maintained. The action of the skin, evidently the chief natural eliminator of the rheumatic poison, requires no promotion in this affection. Frequent purging must be avoided; for the partial exposure to cold, which is its necessary consequence, and the extreme pain and suffering which movement is apt to occasion in these cases (when the co-ex-

isting inflammation of the joints is severe), more than counterbalance, by the nervous excitement they produce, the good which the purging might be supposed otherwise to bring about, as a process of elimination.

"In every case of pericarditis, whether rheumatic or non-rheumatic, and at all periods of the inflammation, the condition of the heart's powers should be carefully watched. Light and easily-digestible nourishment—milk and broth—should be administered during the earliest periods of the inflammation; and stimuli, when the acute period has ceased, and the process of absorption has commenced, or whenever the signs of enfeebled circulation begin to show themselves.

"Of the treatment of the secondary disorders which result from the pericarditis through injury done to the heart's structures; of congestions of lungs, abdominal organs, and brain, &c.; I shall speak hereafter, under the head of valvular diseases."

Further Observations in several parts of Surgery. By Benjamin Travers. To which is appended an Original Memoir or Review of the Nature and Treatment of some Unusual Forms of Eye Disease, by the late Benjamin Travers; dated, 1828. London: Longman, Green, Longman, and Roberts. 1860. 8vo, pp. 205.

The work whose title heads these remarks consists of two parts; of which part one is the produce of the author's pen, containing observations on "Unusual Forms of Injury of the Hip-joint," "History of a Dislocation of the Left Thigh-Bone into the Sciatic Notch," "Observations upon the Nature and Treatment of Boil and Carbuncle," "Clinical Remarks on Hernia," "A Chapter on Hemorrhage, and the Means to be employed for its Suppression," "A Case of Tetanus," "Diseases of the Breast"—this last apparently being a review of M. Velpeau's celebrated work on this subject; whilst the second part contains a letter from the late B. Travers to Dr. Bauer, on some forms of ophthalmic disease. This letter we shall dispose of by remarking, that, though desultory in its nature, it contains, like all the productions of its distinguished writer, much to interest a profession already under deep obligations to him for some of the most philosophical and practical writings in our medical literature. Our remarks on part one, however, cannot partake of the same general character: it will require a more searching investigation at our hands, not so much, perhaps, with reference to the topics of which the author treats, as to

the incidental remarks made almost, as it were, accidentally by him,in discussing these several subjects already enumerated.

In his introductory remarks, we read as follows:—

"*Objections to Excision or Resection of Joints.*—In the early stage this kind of mutilation is inadmissible. 1st. Because the constitution is unimpaired, and is accessible to the impressions of milder modes of treatment. 2nd. Because a large proportion of cases recover, if carefully and skilfully handled: a partial or even complete anchylosis being the common consequence of such efforts. 3rd. Because a system or habit of body which cannot bear milder measures is in no sense calculated to sustain the shock of so violent a proceeding; to say nothing of the demands subsequently created in view of a tedious healing process. Consequently we find that few of the successful cases survive long, to say nothing of those who sink within ten days or a fortnight after the operation of excision. Whatever may be said to the contrary by interested witnesses, I believe it will eventually be admitted, in the face of an unvarying experience, that these docked or curtailed members are comparatively useless. In the lower extremities certainly. The best apology or substitute for the old joints is offered by some of the resected fore-arms. These remarks are perfectly consistent with the fact, that where all the tissues are or have been diseased for a lengthened term, pieces of bone may be picked away, sequestra removed, or a sinus may be dilated to any reasonable extent often with advantage. The principle for which I contend is this: that anticipation and untimely meddling on the part of the surgeon are never justifiable, and always hurtful in the end. The disease in the articulation is a natural though a morbid process. When nature is ready by ulceration or necrosis or a sloughing of soft parts, to cast off a deadly incumbrance, then remove it with as little cost to the system at large as may be; but it can never be necessary, with this simple end in view, to perform an operation, compared with which amputation is a trifling infliction, in a vast proportion of otherwise remediable cases."

We have here quoted from Mr. Travers at full length. This is too important a subject to be at the present day treated lightly; the question is one fraught with too much either of weal or woe to suffering humanity for us to allow so sweeping a condemnation to pass unnoticed in our pages—pages in which this class of operation has been fully discussed, and in which singular ability has been displayed in advocating under suitable circumstances its expediency—nay, its vast utility, and the great boon it confers on sufferers, who would otherwise be subjected to disfigurement and mutilation. The whole question is one that Mr. Travers himself has curtailed within extraordinary narrow limits—"Few of the successful cases sur-

vive long, to say nothing of those who sink within ten days or a fortnight after the operation of excision." Again—"Whatever may be said to the contrary by interested witnesses, I believe it will eventually be admitted, in the face of an unvarying experience, that these docked or curtailed members are comparatively useless; in the lower extremities certainly." Now, without pausing to dwell on the extremely ungracious, nay, unprofessional sneer conveyed in the words, "*interested witnesses*,"—an expression as unworthy as it is disparaging, and one that we should not have expected from a gentleman of Mr. Travers' position, even had he not expressed himself in his preface as anxious not "to hurt the reputation of others"; though how he could more effectually stab the reputation of the distinguished advocates of this plan of treatment, from the earliest, Messrs. Park and Filkin, down to the latest, our own talented and distinguished fellow-countryman, Mr. Butcher, we know not; but the long roll of distinguished surgeons who have more or less identified their names with this mode of operative procedure—including such men as Park, Filkin, the Moreaus, Mülder, Fricke, Jaeger, Roux, Crampton, Syme, Fergusson, Jones, Mackenzie, Erichsen, Canton, Butcher, G. Porter, Tufnell, &c., &c.—requires not their names to be vindicated from so opprobrious a charge by our feeble pen. We shall therefore dismiss this most unpleasant portion of the subject from our consideration, and proceed to consider Mr. Travers' other allegations, which may fairly be epitomized as follows:—

1st. That of the successful cases, few survive long.

2nd. That the non-successful cases die within ten days or a fortnight after the operation.

3rd. That experience proves that, in such cases as survive the operation, the limb is comparatively useless; in the lower extremities certainly.

Now, these three are each in themselves serious charges, not to be lightly hazarded; so serious that, if established on sufficient data, the question of the impropriety of the operation would be at once set at rest for ever. But our readers may well ask what data are furnished us by Mr. Travers to decide this all-important question; and we fear much that they will scarcely credit us when we state that not one fact, case, statistical table, nay, one figure, is brought forward to justify him in this sweeping denunciation of a line of treatment supported by such distinguished operative surgeons as those whose names we have already quoted for our readers. His simple statement, his unsupported opinion, is to weigh more with the rising ge-

neration of surgeons than the recorded cases, facts, and statistics on the subject, with which our medical literature is now so rich. With reference to the first of his three propositions, we should like to be informed why or wherefore a patient who was convalescent after this operation should be more likely to sink into a premature grave, than after any other operation of equal magnitude. Where has he found any records to justify a statement so contrary to what might *à priori* be expected to be the true condition of facts? Were we not in a condition personally, on our own acquaintance with the true statistics bearing on this point, to give this axiom of our author's a most unqualified contradiction, we might rest content with this view of the question; but we do not think that it would be just, in discussing the merits of so important a measure as this, to base our arguments on any *petitio principii*. We will go further, and state that, from opportunities afforded us of witnessing and examining patients who have undergone this operation, we are in a position to assert, that so far as their future prospects of life are concerned, with reference to the resection of their diseased joints, they are as likely to live as any other members of the community; and that, from their present state of health, they are justified in entertaining sanguine expectations of a hale old age, relieved of the disease with which their life had been threatened. This statement may be objected to, as being but the expression of opinion of an anonymous writer. Well, be it so; but are there not diffused throughout the pages of our medical journals remarkable and repeated instances, verified by the signatures of their reporters, of the truth of this position? Is the united testimony of such men as Fergusson, Butcher, Erichsen, Brotherston, Humphry, Jones, Smith, &c., &c., all to go for nought, in deciding such important questions? Why should an operation, if anything involving less serious injury to the human organism than amputation in even the lower-third of the thigh—where, if performed with but ordinary care, the principal vessels are not wounded, and consequently less loss of blood is entailed, and the chances of secondary hemorrhage are to a very extraordinary extent diminished—where the line of incision is removed to a still greater distance from the trunk, where large veins are spared injury,—in fact, where the wound itself is of less magnitude than that of any ordinary amputation in the lower-third of the thigh—why, we ask again, should this operation, *à priori*, and in the face of documentary evidence of the very strongest and most convincing character to the contrary, be accused of being

ultimately more fatal than amputations under similar circumstances?

As to the second proposition, involving the death of the non-successful cases within ten days or a fortnight, we should like to ask Mr. Travers at what period, or how soon after amputation, do the fatal cases occur? But to meet him on this point would, in our minds, be but a fencing with a question of serious import to the community at large, and the scientific position that conservative surgery now occupies in these islands. Were we but discussing this question as between ourselves and Mr. Travers, we would at once end the discussion, by referring him for perusal and edification to the masterly essays on this subject that have appeared in the pages of this Journal from the pen of one far more capable of doing justice to this or any surgical topic—the uncompromising advocate and able exponent of this form of operation, Mr. Butcher. But as Mr. Travers, in his preface, states that his object is to secure for himself "the credit of having striven in his generation to arrest the progress of error and enlarge the bounds of truth," we, with a similar laudable intention, shall be permitted to extract from Mr. Butcher's masterly essays the following summary of cases, amply and ably recorded by him for a similar object:—

In Mr. Butcher's first memoir on excision of the knee-joint, we have, in a tabular arrangement, an account of thirty-one cases, of which five were fatal, and twenty-six recovered; and in his second memoir on the same subject, fifty-one cases are recorded, of which number ten were fatal, and forty-one recovered; making a sum total of fifteen fatal cases, and sixty-seven recoveries, out of eighty-two cases subjected to the operation. From the number of recoveries, however, we have to subtract six cases, in which amputation was had recourse to as a secondary measure, which will leave on record a sum of sixty-one recoveries, as against twenty-one failures; or, in round numbers, we may state that three-fourths of the number of cases operated on, or seventy-five per cent., were successful, and from most trustworthy sources proved to be so. And yet we find Mr. Travers, in the year of grace 1860, coming out with the sweeping general assertion already quoted in these pages, and not vouchsafing us one figure, one fact, nothing beyond his own mere *ipse dixit*, to support his statement and deliberately recorded opinion. We care not here to pause to inquire, might not a more careful analysis of these unsuccessful cases, with a view of attributing the fatal issue to its first cause, be attended with the result of even yet more reducing

the number of those fairly to be attributed to the operation itself. We seek not (though in justice, were we discussing the merits of the operation, we would be bound to do so) to inquire how many of them died from pyœmia, extensive bedsores, organic disease, hemorrhage, phlebitis, shock, delirium, &c.—all of which complications tend to swell the fatal numbers of even the best accepted and most recognised surgical operations. We, for argument sake, will grant that each and all of them are with justice to be ascribed as a natural consequence of the form of operation selected; and still we assert that it will bear favourable comparison with amputation of the thigh. In this operation the mortality is scarcely twenty-five per cent: has any one authority in surgery ever yet assumed as low a rate for that succeeding amputation?

We shall now proceed to examine how correctly Mr. Travers has stated that "experience proves, in such cases as survive the operation, the limb is comparatively useless; in the lower extremity certainly;" and, to enable us to do so, we shall again have recourse to Mr. Butcher's most valuable and masterly analysis of the result of this operation. In thirty-one of the cases in these tables, we find these words appended, "WITH PERFECT USE OF THE LIMB"; and this exclusive of many others exhibiting a most satisfactory condition of the limb, leaving but little doubt on any candid or unprejudiced inquirer's mind, that they also were, or shortly would be, additional triumphant examples of the extraordinary boon conferred by modern conservative surgery on these forms of disease. Thirty-one cases, in all of which under the old *regime*, the unhappy being would be a maimed object for the rest of his life, even if that life had been spared in the fearful game of equal main and chance which had been played for him, but in which now the patient has a *perfectly useful limb*, has the symmetry of his body preserved, and is not dependent on mechanical skill for the means of progression, or the preservation of his appearance[a].

Some people, however, Didymus-like, require to have their senses convinced, and will not accept anything on mere assertion. Surely such proof was easily to be procured for Mr. Travers' sceptical mind; the brilliantly successful cases of Professors Fergusson, Erichsen, Bowman, &c., &c., were open for investigation, and *should have been examined, and carefully examined*, ere he dare to venture to question the merits of an operation now firmly established on such high authority and such unquestionable statistics. Such of us as have had in this city the opportunity of seeing and examining Mr. Butcher's

[a] See also Dr. Canton's first case, at page 76 of our present Number.

most successful cases, Mr. George Porter's most interesting case of excision of the elbow-joint for injury, and Mr. Tufnell's case also of a similar operation for an analogous accident, will not allow our judgments to be warped by any rash, ill-judged, and unfounded criticism on the merits of this form of operation; but to such as have not been so fortunate, it becomes our duty to warn them to reserve their judgment, to be most cautious in condemning an operation of which personally they have had no experience, and of which consequently they are only entitled to form an opinion from the writings and statistics of those who have studied the question, and are competent and entitled to speak on it with authority—to place for one moment in serious juxtaposition the advantages to be derived from an operation that preserves in their perfect integrity the motions of all the articulations below the one that has been excised, and that in which all these, together with the diseased joint, are sacrificed, is simple inanity; whilst, to claim *superiority* for that in which artificial means must supply those of nature, is but to assert that the work of man is greater than that of God. The question can, nay, ought, only to be discussed on this one great ground. On which side do statistics prove the greater mortality to exist? And we think that we have justified ourselves to our readers in stating that this point has been decided on most abundant and satisfactory evidence to be in favour of excision.

That a gentleman who has taken exception to the propriety of the operation of resection of joints—an operation of English growth, warmly advocated, extended, and improved by English surgeons, aided, as we feel they will gratefully acknowledge, by Irish talent and industry,—should attempt to cry down one of merely Irish growth, is not to be wondered at. Therefore our readers must not be surprised to hear that the treatment of aneurism by compression comes also under Mr. Travers' lash. We are so anxious to avoid even the semblance of injustice in this gentleman's case that we shall submit his own words for examination:—

"*Magna est veritas!* The universal adoption of the ligature as a cure for aneurismal swellings, and the comparative certainty of its success in properly selected cases, is the best excuse which can be offered in explanation of our views as pathologists; and it justifies the conviction now commonly avowed by all operators in this country, that neither the presse-artère, nor any amount of external compression, whether mediate or immediate, is worth the pains and risk attendant upon such indirect and comparatively imperfect modes of procedure. Exceptions, however splendid, never justify a rule."

It is true that this lecture was delivered so long back as 1847 to the surgical class of St. Thomas' Hospital; but can this plea in abatement be urged with any justice for the statement being put in print in the year 1860, without one word of explanation or qualification? Before 1847, the entire surgical profession in these countries had ample opportunities afforded them of studying the relative merits of ligature *v.* compression. Bellingham had written and taught, as also Tufnell, on this most important subject; numbers of cases had been given illustrating the vast advantages of compression as contrasted with deligation; and at the present hour to question in suitable cases the respective values of the two modes of procedure, is worthy of those alone who write as Mr. Travers has done on resection. The only excuse for his so doing is, as we have already intimated, that the lecture was delivered at a period when English surgeons were not unanimous on this great Irish improvement; for even in these days of steam and telegraphs, it takes a long time for a fact to reach St. Thomas' Hospital from the City of Dublin or St. Vincent's Hospitals; but to write now as he has expressed himself in this passage puzzles our inventive faculties for explanation. It may have been done with the view of giving us an historical example of the difficulty under which truth labours for its diffusion: just as Stephenson's "Coo" is quoted in works recording the obstacles thrown in the way of the introduction of our present system of railroads; but what would any child of the present day think of the engineer who would gravely advance as an argument against a locomotive the danger that would accrue to *it* did a cow crossing the line come in contact with it? We rather think that he would be inclined to agree with Stephenson that it would be "a very bad thing—for the coo." We repeat that the objection is of interest in an historical point of view; and so likewise would be Mr. Travers' reference at the present moment to an opinion expressed thirteen years ago on a point of surgical treatment at the time with many able English surgeons still *sub judice;* but gravely to repeat it now, argues a want of progression with the science of surgery that would be laughable, were it not at the same time so very much to be deplored in the person of one aspiring to teach our future race of surgeons.

It is not our intention to attempt the vindication of the treatment of aneurism by compression. At the present day, the surgeon who questions the value of this plan of treatment cannot be allowed to do so on his mere assertion: facts and arguments will be looked for by surgeons at his hands; and if

he be not prepared with such, his opinion will be just received at as much as it is worth. But we cannot resist reminding our readers of a passage that occurs in Mr. George Porter's most valuable "Contributions to Operative Surgery," that appeared in our number for last November. This passage, extracted from the latest production on the subject, and emanating from the pen of a surgeon that all who have witnessed his practice in the surgical wards of the Meath Hospital will acknowledge not to be timid or unskilful in the use of the knife —that "last appeal" of every sound surgeon,—must be accepted as the exposition of the surgical treatment of this most important class of diseases at the present period:—

"In the meantime, a case of popliteal aneurism was cured in the Richmond Hospital by mediate compression; another occurred in Jervis-street Hospital; and soon this method of treatment came to be ratified and confirmed by a course of success so unvarying and continued, that most surgeons began to regard the operation of securing the femoral artery as little more than a matter of bygone history, never more to be resorted to in the treatment of popliteal aneurism.

"But in medicine there is no proposition universally true, —no mode of treatment applicable to every possible case. Doubtless, the operation of deligating the femoral artery at the usual place has totally fallen into abeyance, and probably has not been twice performed in Dublin during the last fifteen years; whilst the treatment by compression has been generally so successful, and has so entirely won the confidence of the profession, *that very good and sufficient reasons would be required of any practitioner proposing to deviate from it.*"

The paper on anthrax is written to advocate the value of the exclusive use of potassa fusa in the treatment of this form of disease. As an auxiliary, we are willing to accept it; but to depend on it, to the complete exclusion of other modes of treatment, is what no sound surgeon will consent to do. The entire subject of anthrax and its treatment, however, has been some short time back (in our number for November, 1856, vol. xxii.) so ably handled, in a most masterly paper on the subject by one whose untimely loss Irish surgical literature is still deploring—Mr. T. H. Ledwich—that we must content ourselves by referring our readers to a reperusal of that memoir, in which they will find this and other plans of treatment contrasted and discussed in a most exhaustive manner, and for which Mr. Travers' present paper would indeed prove but a very poor substitute.

In heathen mythology, the god Mercury has been accused

of many a trick, and had many a sin laid at his door, but we doubt if his greatest detractor in former times ever brought a stronger charge against him than this of Mr. Travers:—

"I knew a very fat, jovial woman, fresh-coloured, past 40 years old; she had borne several children, and never had an hour's illness in her life. One day she showed her ordinary attendant a small hard tumour in her left breast, which was plump and well-formed. The induration did not extend beyond the lump, which was as hard as a stone, and moved freely in the substance of the mamma. Amongst other remedies, this patient was ordered to try a five grain plummers' pill every night and sarsa. She had not taken more than a dozen pills when she was attacked with acute pleuropneumony, of which she died rapidly. I saw the case myself, and I observed at the time that I thought the mercury had killed her."

Many ills have we known to arise from the injudicious use of mercury; many woes have been attributed to its abuse; but gravely to ascribe a fatal attack of pleuropneumony to the dose mentioned here, is certainly rather too severe a tax on our credulity. The *post hoc propter hoc* theory never has been better illustrated than in the present instance.

In concluding our notice of Mr. Travers' work, we must express our regret in having been forced, in the strict discharge of a public duty, to speak of it in such harsh terms. We opened it with a most favourable impression, impressed with the author's hereditary claims on the respect and kindly feelings of every true lover of surgical science; but we are much deceived in the estimate we have formed of the character of the late Mr. Travers, if he (were he compelled to criticise it) would not himself, in vindication of surgery as a science, act with as stern a sense of duty as in former times, in defence of social order, actuated the Warden of Galway.

The Surgical Diseases of Children. By J. COOPER FORSTER, F. R. C. S., &c. London.—Parker: 1860. 8vo. pp. 348.

WHEN an author brings out a work on a practical subject, the first inquiry we naturally make is, what his opportunities have been for acquiring experience. From the title-page of the book before us, we perceive that Mr Forster is Assistant-surgeon to Guy's Hospital, and Surgeon to the Royal Infirmary for Children, so that we may reasonably expect him to be well qualified for the task he has undertaken. The surgical wards of

Guy's Hospital receive, we believe, more than three hundred children annually, and by children patients under puberty are meant. In the preface we are told that "the entire matter of this work is original,"—that it contains the result of the author's own experience; and where his own observation does not afford any data, he is silent. It is not his aim, then, that this should be a complete systematic treatise upon the surgical diseases of children; and accordingly we find some subjects altogether omitted, as diseases of the eye—and others, as hernia, and injuries of the head, &c., but slightly dwelt on, as not presenting in the case of children any differences either in character or treatment from the same diseases in the adult. Above, when it is stated that the matter of the writer is "original," it is not to be supposed from this that the author's views and modes of treatment are new or original—in this sense, there is little of originality in the book—but merely that he tells us the opinion he himself entertains, the principles that have guided him, and the plans of treatment he has himself followed, irrespective altogether of their originators, and without any reference to "authorities." To our minds, a book of this kind is much more attractive than any compilation; and, provided its author be a man of information and experience, is much more profitable to the practitioner.

In all operations, even of the slightest kind, Mr Forster administers chloroform, considering it to be so safe in the case of young children. He believes that as children advance in years. the danger of chloroform increases; but he admits to having heard of a "very few" fatal results, when employed in early life. He very properly enjoins that it should be given on an empty stomach, to avoid sickness; and the sensitiveness of the conjunctiva is the test on which he relies, for indicating when the patient is fully under its influence. Freezing mixtures (for producing local anæsthesia) he considers inapplicable for very young children; but that in children verging on puberty, this agent may be most advantageously employed, in the opening of abscesses, and other cases in which it is desirable to abolish the cutaneous sensibility. As this kind of local anæsthesia is not as much employed, we think, as it might be, we here borrow Mr Forster's directions.

"The best mode of applying cold is to mix thoroughly together equal parts of ice, broken into very small pieces, and of salt, and place them in a piece of very thin caoutchouc sheeting. This should be gently drawn to and fro over the part for two or three minutes, until the skin begins to assume a whitish appearance. In this state, if the skin be incised, it cuts like a raw turnip and without the

least sensation to the patient. Some little uneasiness attend the first application of the freezing mixture, and also its withdrawal. The latter may be somewhat relieved by making the withdrawal gradual. To avoid sloughing, care should be taken that the application be not continued too long."

We cannot possibly notice all the different subjects treated of by Mr. Forster in the pages of this work; indeed, they comprehend nearly all the diseases and accidents of early life, calling for surgical treatment. Some of them, however, we shall notice, in order to give the reader the benefit of his remarks, or to make them the subject of criticism.

Epistaxis, though a very common, and sometimes a serious complaint, yet, in the experience of Mr. Foster, has never proved fatal, or even dangerous; as on no occasion has he ever had to employ plugging of the nostril.

Cancrum oris he regards as a disease distinct from gangrenous stomatitis, with which it is often confounded, and under which appellation often described. But when we find so high an authority as Dr. West so designating it, it may well be supposed that the difference, if any, between the two complaints, is very slight. The question of whether, or how far the administration of mercury may have to do with the production of this terrible malady, has, we believe, been decisively settled in the negative. That a gangrenous affection of the gums and cheek has supervened upon the use of mercury with children, is an undoubted fact, but one of excessively rare occurrence, and attributable to accidental coincidence, or to a peculiar idiosyncrasy of the patient, just as the same destructive process may attack adults using this mineral. In the last (28th) volume of the former series of this Journal, Dr. James Duncan has published a most able essay on the disease, and has fully entered into this question; and he has produced a large amount of evidence tending to prove, in the most conclusive manner, that cancrum oris may, and most frequently does, arise altogether independently of the use of mercury. In opposition to all this, Mr. Forster "is disposed to believe that the indiscriminate use of mercurial powders, a portion of which is very frequently retained in the mouth of young children, is, in the larger proportion of cases, the exciting cause" of the disease; whilst previous depressing maladies, badly-ventilated dwellings, poverty, dirt, starvation, &c., &c., he regards as predisposing causes.

Hare-lip.—With respect to the time for remedying this defect by surgical treatment, the author's experience is decidedly in favour of "operating at the earliest possible period; remembering, however, that loss of blood in very early infancy

may be attended with the worst consequences." He would have no hesitation in operating immediately after birth, and has successfully done so within seven hours. His principal reason for preferring to incur this greater risk from hemorrhage, in the very early operation, is, that the child may be enabled to suck. The danger from the operation he believes to be less than that from the imperfect nutrition which results from the deformity. In the main, he is right, we think; and the large majority of these children are physically in a much better condition for undergoing the operation soon after birth, than for months afterwards; nay more, in some of these cases, where the deformity is very great, at no subsequent time is the child in such good condition for being operated upon; and, in a few instances, if this period be allowed to pass over, the opportunity for operating is irretrievably lost; for the child so rapidly falls away in health, that no prudent surgeon would think of subjecting it to any serious operation. Mr. Butcher—no mean authority upon this subject—gives a preference to the early operation, but "prefers waiting a few days after birth, in order, as it were, to allow the functions of the body to be healthily in action[a]." This reasoning is, perhaps, rather fallacious; for, in nineteen cases out of twenty, children, on their entrance into the world, are in far better bodily health than they are for a long time afterwards; and, in corroboration of this, we would mention the fact, that nearly all infants lose weight the first week after birth.

The various accidents to the larynx and trachea, which are apt to require tracheotomy, are clearly described, as well as the operation itself. Upon the much-debated question of the utility of this operation in croup, Mr. Forster offers no opinion, very cautiously declining to enter into the controversy. This reticence on his part we much regret, and rather deprecate. As the author professes to give us in this work merely his own views and experience, it was reasonable to expect an expression of his opinion on this great practical question. That he has had considerable experience of the measure is plain, from the little information he vouchsafes relating to it. "I have performed the operation (he observes) in various periods of the disease, and with varied success." As to the particular period at which it should be performed, he thinks that the advanced stage of the disease is the best time; nay, he even "doubts much whether when a patient is moribund be not the most advisable time for the proceeding;" and, in illustration, reports

[a] See his paper on Hare-Lip, in the twenty-first volume of this Journal.

the case of a boy four years old, on whom he operated with complete and permanent success, and yet the symptoms presented by the child before the operation were such as indicated immediate dissolution—perfect insensibility, face and lips quite livid, faint efforts at respiration, but the air scarcely entering the chest, the pulse imperceptible, and the skin cold. This case is entitled "Laryngitis;" and, perhaps, the question may be raised, whether it really was an example of genuine croup; certainly it does not appear that any false membrane was formed in the trachea or its subdivisions.

Diseases of the urinary and generative organs, in both sexes, are discussed at considerable length. These chapters, four in number, display much practical knowledge, and will be consulted with profit and advantage by all surgeons who are liable to be called in to attend upon the young; and this takes in, we believe, all *civil* surgeons.

Infantile Leucorrhœa.—In connexion with this disease, the author alludes to the frequent supposition of its being of a specific kind, and communicated by the foul contact of some person affected with venereal disease. He admits the impossibility of distinguishing gonorrhœa, by any pathognomonic symptoms from some cases of infantile leucorrhœa; but, at the same time, he is of opinion that, as a general rule, not the slightest importance is to be attached to the allegations of the friends of the child as to the origin of the complaint, and that it is certain circumstantial evidence only, of the most unequivocal kind, can lay any basis for the imputation. The statements made by the children themselves are entitled, he thinks, to very little reliance. In all this, he only corroborates what has been written on the subject by others, and especially by Dr. Wilde, to whose papers he alludes in terms of strong commendation.

Calculus in the urethra.—Apart from external injury, this is almost the only cause of retention of urine in the child. In these cases, the inability to pass water is the first symptom; there is no history such as we find in stone in the bladder. Generally speaking, by the time the surgeon is called in, the bladder is considerably distended, and may be felt above the pubes, and through the rectum. In such a case as this, the treatment is, according to our author, "first of all, to place the patient under the influence of chloroform." On this, and another point, namely, the mode of removing the calculus from the urethra,—we must let Mr. Forster speak for himself:—

"Only actual experience can enable us fully to appreciate the value of chloroform, and the instantaneous relief it gives, not only

in some instances of retention of urine, but in numerous other affections of children. It is not, however, always the case that the calculus is expelled on the administration of the drug; and if the retention continues, a catheter must be passed. In doing this, the calculus may be distinctly felt, and accidentally pushed back into the bladder, where its presence may, or may not, be afterwards detected by the sound. . . . .

"If not contained within the prepuce, the calculus may be found at any other part of the urethra, but most frequently about the bulb. Various plans may be adopted for its removal, according to the part of the canal at which it is lodged. If just within the meatus, the orifice may be slit open, and the calculus abstracted. If situated in any other part of the urethra, a long narrow pair of forceps has been recommended to be passed down, and the stone seized. This, however, is an unsatisfactory proceeding in children; and I have never adopted it, believing that it is fraught with less danger and injury to the canal to pass a grooved staff down to the stone, and cut into the urethra in the mesian line from the perineum, or from the under part of the penis, either in front of, or behind the scrotum. I must advert here to a maxim that is inculcated, but which I am satisfied is incorrect, viz., that the urethra should never be opened in front of the scrotum. I have opened it there myself, and seen the same thing done by others, and the wound has healed quite as rapidly as when the incision was made in the perineum."

One entire chapter is devoted to the subject of calculus in the bladder, and a description of the operation of lithotomy. It is full of sound practical instruction, and may be read with advantage by all. Indeed, these chapters on the diseases of the genito-urinary organs (especially in boys) are among the best in the book, and give the results, in very plain language, of large clinical experience.

Paraphymosis.—The author's plan for replacing the prepuce, in this disease, is "to place the boy on his back, to grasp the penis firmly with the right hand, and proceed to lift him bodily up." He has found this mode of reduction so successful, that he now resorts to no other. We think that the inhalation of chloroform would be very proper, before subjecting the child to so painful a proceeding.

Nævus.—Passing by the chapters on diseases of the testis, and on hernia, we come to that one on nævus, and feel bound to say that it contains a very comprehensive and explicit description of this disease, and of the different modes of curing it, with occasional remarks upon the comparative value of these various plans. Departing from the course which he elsewhere pursues in his work, viz., of strictly confining his observations

to the results of his own experience, he here gives a very good and clever resume of all the methods (about sixteen in number) at present in use for the cure of nævus. As we have heard a good deal lately about the injection of nævi, we shall extract some of his remarks thereon:—

"Until the recent introduction of the perchloride of iron into surgical practice, by Malgaigne, the operation of injection for the cure of nævi was comparatively unsatisfactory. This substance, however, if used in certain picked cases, is so efficient, and with proper precautions is so safe, that it has, in my opinion, given the procedure by injection a place among the best remedies for this affection. The cases, however, must be carefully selected, or disappointment will ensue. When the nævus is of the true subcutaneous type, the skin not being involved, and when it is situated in a conspicuous part of the face, neck, or arms, where a scar is to be avoided, injection of the perchloride of iron is the plan I always adopt. But these cases, it must be remembered, are not frequent. The mode in which I perform this operation is as follows:—First, introducing through the skin a narrow double-edged knife, about half an inch from the apparent margin of the nævus, I pass it into the substance of the growth, and move it in all directions to divide the tissue. If the nævus be large, I do this from several points. As the knife is withdrawn, there is a flow of blood, which the pressure of the finger will control. Then, the syringe being previously charged with the solution of perchloride of iron, I introduce the point of it into the aperture, or into each aperture in turn, if there are more than one, and throw into each opening about four or five minims. Coagulation of the blood instantly takes place; and the tumour becomes hard and prominent. Absorption gradually ensues, and the structure is destroyed. If the injection penetrate the whole nævus, as evidenced by the hardening of the whole, no further operative procedure will be necessary; if, on the contrary, any portion retain its softness, that part should be injected through a fresh opening, as soon as convenient. When the absorption is complete, no trace is left to show where the disease has been; but this process may occupy several months, or even a year."

The succeeding chapters of the book are upon injuries and diseases of the bones, joints, ears, &c., diseases of the skin and nails, congenital deformities and malformations, tetanus, burns and scalds. Though the remarks on these are generally very brief, yet they are practical, and to the point.

From the extracts we have given, and the comments already offered, the reader can be at no loss to know our opinion of this work. Although this is, on the whole, most favourable, still we think that on some subjects the author might advantageously extend his remarks; whilst some of the subjects on which

he has treated (e. g. diseases of the skin, infantile syphilis, scalds, burns, &c.), might be omitted altogether, not presenting sufficiently distinctive features in children to merit notice in a work of this kind. We cannot close this notice of Mr. Forster's work, without mentioning that it is abundantly illustrated with well-executed wood-cuts and coloured lithographs.

The Principles and Practice of Surgery. By William Pirrie, F. R. S. E., Professor of Surgery in the University of Aberdeen, &c., &c. Second Edition. London: Churchill, 1860. 8vo, pp. 880. Illustrated by numerous engravings on wood.

Amongst the books which have been issued from the press during the past month, we have the second edition of Mr. Pirrie's treatise on surgery. At a glance, the volume is an attractive one; but when we come more closely, more particularly to study and analyze its contents, we are struck by the concise, practical way in which the different subjects are handled. Eight years have just elapsed since the appearance of the first edition; and certainly the author seems not to have spared time, labour, or money in enlarging, illustrating, and bringing out a work in every way commensurate to the wants of the student, and furnishing an admirable epitome of the surgery of the day. Many chapters have been entirely rewritten, and others greatly enlarged and improved. On contrasting the bulk of the volume with its predecessor, this may not appear; but on nearer examination it will be seen that the type is much smaller, and more closely set; while the most beautiful woodcuts adorn almost every page, each of them being a faithful exponent of the text. We could not afford room to enter into the merits of the different sections into which the volume is divided, as otherwise we would be inclined to do; but this is the less required—indeed, the necessity for so doing is negatived in regard to a book which has passed so rapidly through its first edition. Each chapter, according to the import of the subject, seems to have called forth the energies of the writer; and, as a sequence, there are some to which we would wish particularly to direct attention, as being replete with information, condensed in the most suitable way. For instance, the exposition of the subject "Aneurism" is clear, most instructive, and perfect throughout. And the same

truthfulness which stamps the book, accords without any petty, narrow-minded jealousy, the credit and the honour which redound to the Irish school for the establishment of its treatment by compression, as a practice which, in the author's language, "is a safe, simple, successful, and almost painless mode of curing aneurism." He justly associates with Bellingham's name the important truths revealed in its cure:—

"When Bellingham first called attention to this interesting subject, he stated as his opinion, that it would be unnecessary to employ such a degree of pressure as would cause inflammation and obliteration of the artery at the seat of pressure; but that it would be sufficient merely to weaken the circulation through the artery and the sac, thereby favouring consolidation by the deposition of lamellated coagulum. In cases treated successfully by this mode, opportunities have occurred of making *post-mortem* examinations, in consequence of the fatal results of other diseases; and it must have been gratifying to Bellingham to find, that in most of these instances the main artery was pervious everywhere except at the aneurism."

And again:—

"The late Dr. Bellingham, one of the surgeons of St. Vincent's Hospital, Dublin, had the merit of having suggested this new mode of using pressure, and of having proved its success, as well as of bringing the subject before the profession. He treated a considerable number of cases with perfect success, and his method has been practised with equally gratifying results by other surgeons in Dublin and elsewhere. Bellingham stated, in regard to the favourable impression entertained of this mode of treatment, 'So highly satisfactory has been the result of compression in Dublin, that no surgeon of that city would in the present day perform the operation of applying a ligature to the femoral artery for popliteal aneurism.'"

It would be unjust to the clear discernment of the author, if we did not quote his concluding sentence in reference to this subject. It may be taken as a test for his truthfulness in other statements:—

"The surgeons of Dublin can have no possible motive for preferring compression to operation, except the conviction that it is the preferable mode of treatment. In operative surgery, 'whatever men dare, they can do,' as well as hands can do; but in suitable cases they prefer compression to operation, because they believe it to be the safer mode of treatment."

The subject of phlebitis, the very name of which sounds alarm in the ear of every surgeon, whether it steals on insidiously as an idiopathic affection, or suddenly strikes with

death the sufferer after operation, is likewise dealt with in a masterly way. All that is known about it—its cause, its course, its management, its pathology, have each been considered with research, acumen, and judgment.

There are other chapters, also, to which we would wish particularly to refer. That on hernia is especially interesting and practical in all its details; and this section is most elaborate, occupying nearly fifty pages.

We have a concise but brief account of resection, this conservative effort of our art, as applied to several articulations. The author gives the result of two cases of excision of the wrist-joint, occurring in his own practice, attended with the best results:—

"I have twice performed the operation of excision of the wrist-joint; once in the Aberdeen Hospital, and once in private practice. In the latter case I operated according to Mr. Butcher's method, and the result was a serviceable hand, with three of the fingers decidedly bent. In the former case, I made a long and straight incision behind, deflected the tendons to each side, and removed the whole of the carpal bones, along with the extremity of the radius; and the gratifying result was, that the patient had a most serviceable hand, and enjoyed the full use of all her fingers."

From the careful way in which this second edition has been produced, we have no doubt that it will shortly be exhausted; and we look forward to a third edition as the immediate consequence. No book in its early editions can come up to perfection; so far as it is possible, Pirrie's Surgery will take its stand now. Within the last few years, surgery has made such rapid strides, and is still so progressive, that the author will constantly be apprized of the necessity for additions; and we shall look forward with confidence to the third edition of this valuable work, and put our trust for the consummation of our wishes in the zeal and ability displayed in this second edition. The clearness and precision of Professor Pirrie's descriptions have thrown a charm over the entire work; and the honest high-mindedness with which he accords to every man what his researches and labour may have brought to practical utility and worthy of notice, calls for our highest commendation; it being too much the custom of authors of systems of surgery, in the present day, to seize upon the opinions and writings of others, and put them forth clothed in their own language, without reference to a name, or acknowledging the sources from which in reality they were procured.

In conclusion, we can assure the learned author that we

have read with pleasure every page of his book, and heartily recommend it to the student as a volume best fitted to qualify him for those competitive examinations which are the order of the day. But does its usefulness rest here? Certainly not. The practical precepts laid down upon all critical questions must enhance the value of the book, and render it indispensable to the practising surgeon. It should be conspicuous in his library shelf for reference, and we pledge ourselves it will not fail him in his necessity.

On the Reparative Process in Human Tendons, &c. By WILLIAM ADAMS, F.R.C.S. London: Churchill. 1860. 8vo, pp. 175.

IN the twenty-fifth volume of our present series we had occasion to notice, with approval, Mr. Adams' sketch of the "Principles and Practice of Subcutaneous Surgery,"—an essay which, in spite of some trifling faults of style, gave evidence of careful observation and sound reasoning. In the present work, which follows appropriately on the former, we rejoice to observe the same close powers of observation, with an absence of anything disagreeable in the style In it will be found a series of observations on the union of tendinous structures, carefully noted and compared; there is also a profusion of excellent illustrations, both of the physical characteristics and of the minute anatomy of the new material, at periods varying from four days to three years after division. In addition, there is a series of experiments on rabbits, with a resume of the English and foreign literature of the subject. The work should be on the shelves of every operative surgeon who has claims to a scientific acquaintance with his profession. No doubt, it can add little to our knowledge of the art of surgery; but to those who love to go deep into its principles, such contributions are most acceptable. In the hope of inducing such of our readers to become personally acquainted with the work, we copy a few of the observations and conclusions of Mr. Adams. Before doing so, we have to remark, that the previous literature of the subject, commencing with John Hunter, deals rather with experiments on animals, than with observations derived from the human subject. Owing to the wide difference of the results obtained by successive writers, Mr. Adams instituted, in the year 1855, a series of thirty-two experiments on rabbits—of these he remarks—

"I will here only observe that my investigations generally confirmed the account of the process previously given by Mr. Paget, espe-

cially with regard to the influence of the blood and the inflammatory process in interfering with, rather than assisting, the reparative effort; in this respect, therefore, they were at variance with the opinions of all the other authorities above mentioned [Hunter, Mayo, Von Ammon, Pirogoff, Gerstaecker, Thierfelder, and Bauer]. My investigations also confirmed Mr. Paget's observations with regard to the development of the new connective tissue from a proper reparative material, effused for this purpose, and described by Mr. Paget as 'nucleated blastema,' a material in which the cell-forms do not advance beyond the condition of nuclei, and thus differ from inflammatory lymph, in which fibro-cellular tissue is developed from nucleated cells. The most material point in which my observations differed from those of Mr. Paget, was in reference to the influence of the sheath of the tendon, which I found not divided, and generally very little injured, in the subcutaneous operations. It therefore appears to me, that in the reparative process the sheath was of primary importance in maintaining a direct connexion between the divided ends of the tendon. In my experiments, also, it appeared that the new reparative material was infiltrated between the fibrous elements of the sheath, which therefore at once formed the matrix for the newly-formed tendon, and also determined its direction and definite form, as described by Thierfelder.

"Mr. Paget, on the other hand, completely ignores the influence of the sheath in the reparative process; and, after describing the general infiltration and succulency of all the tissues surrounding the tendon, and the subsequent changes which occur in any inflammatory lymph and blood which may be effused, and the manner in which the development of the nucleated blastema laid down between the separated extremities of the divided tendon appears to proceed, observes that 'a single well-designed (? defined) and cord-like bond of union is thus gradually formed, where at first there had been a uniform and seemingly purposeless infiltration of the whole space left by retraction of the tendon.'

"Mr. Paget also states, that in experiments in which the tendon was divided by an open wound, when the wound through the integuments healed quickly, the case proceeded like one in which the subcutaneous division had been made; and therefore he adduces this in proof 'that it is unimportant for the healing of divided Achillis tendons whether the cellular sheath or covering of the tendon be divided or not.' The facts and observations which have led me to a different conclusion on this point, will be specially adverted to in the general description of the reparative process given below."

For these, however, we cannot make room, and shall pass on to the more interesting and unique collection of observations on human tendons.

The tendo Achillis of a child was divided, Oct. 28th, and on the 1st of November the child died of bronchitis. In this case—

"The divided extremities of the tendon were three-quarters of an inch apart; but a direct connexion between them still existed by means of the cellular sheath of the tendon, which appeared to have been but little injured by the operation, and passed between the divided extremities in a tubular form. The sheath was highly vascular between the divided extremities of the tendon; and a close net-work of congested vessels ramified through its structure, which was a little increased in thickness, and slightly succulent in appearance, from infiltration into its cellular meshes. The vascularity extended above and below only for a short distance beyond the divided extremities of the tendon; and the surrounding fat and cellular tissue presented a blood-stained, vascular, and infiltrated appearance. . . . The divided extremities of the tendon were square, abrupt, and unaltered, either in appearance or texture. It might, perhaps, be said, that the edges were slightly rounded; but the general appearance was that of a section just made."

In another case, the little patient sank from diarrhœa, eleven days after operation:—

"The divided extremities of the tendo Achillis were seven-eighths of an inch apart; but were directly connected by a solid bond of union, equal in diameter to that of the tendon, but of soft consistence, and externally of blood-red colour, partly from blood-staining, and partly from vascular injection. The surrounding fat and cellular tissue also presented a vascular and blood-stained appearance, and were infiltrated with inflammatory effusion.

"By a little dissection, I was enabled easily to define the connecting bond of union, the contour and definition of which evidently depended upon the cellular sheath of the tendon, the fibrous bands of which, now taking a longitudinal direction, were readily distinguishable. The substance of this connecting bond presented, on section, a somewhat gelatinous appearance, of a reddish colour, partly from blood-staining, and partly from vascularity. There was nothing like separable blood-clot anywhere, and very little blood appeared to have been effused at the operation in this case. Both the cut extremities of the tendon presented a square, abrupt outline, the upper being only somewhat rounded at its margins. They retained their dead-white colour, and natural texture; perhaps it might be said, that they were slightly softer than natural, and thus contrasted strongly with the blood-stained gelatinous texture of the new connecting material; with this material, however, they were pretty firmly connected, though, to the naked eye, there was no appearance of the dove-tailing process of junction, so obvious in the specimens at a later date."

The microscopic appearances in this new material were those of nucleated blastema. Omitting many other observations of intermediate periods, we pass on to the description of two ten-

dons, which had been divided three years before the death of the patient afforded an opportunity of inspection; here—

"The divided extremities of one of these tendons were found to be an inch and a quarter, and the other one inch and an eighth apart, but firmly connected by a firm bond of union, or new tendon, equal in bulk and thickness, to the extremities of the tendons it served to unite, and presenting a greyish translucent appearance, by which it was readily distinguishable from the old tendon. The new material had, in a more marked degree than I have seen in any other human specimen, a longitudinally striated appearance. The cut extremities of the old tendon were very distinct, from the contrast of colour above described; and, the line of junction with the old tendon indicated by a process of fine dove-tailing. The new material extended, at its circumference, slightly beyond, or, it may be said, slightly ensheathed the extremities of the old."

Microscopically, there was no difference between the new and the old tendon.

In some dissections, Mr. Adams found a remarkable divergence from the above chain of phenomena, in the case of tendons enclosed in dense fibrous sheaths of a tubular form. Where tendons are so circumstanced, as is the case with the tibialis posticus, behind the ankle-joint, there appears to be a danger that the divided extremities will contract adhesion to the inner surface of the sheath, and that no direct bond of union between them, or new tendon, will ever be formed. We have lately seen a case of rupture of this tendon in this situation, in which, at the end of a month, no proper union, or attempt at it, had taken place; and, as far as one case goes, this corroborates Mr. Adams' views.

These observations serve to show that the function of the sheath has much analogy to that of the periosteum in the case of fracture, namely, that it mainly, but not entirely, furnishes the material for repair, and maintains the vascular connexion between the parts; and we have a practical rule deducible, namely, that extension should not be applied too early in the case of tendons which have but slight vascular connexion with their sheaths, as, in such cases, non-union is apt to occur, if there be too great separation of the extremities.

There is a controversy we hope an amicable one, between Mr. Adams and his colleague, as to whether the new material, which both parties agree to be poured out for the repair of divided tendons, remains as permanent new tendon, or gradually contracts until it approximates their ends, and forms a linear cicatrix. The latter theory is not well supported by ob-

servations, but is more in accordance with the general theory of the reparative process. Whereas, Mr. Adams advances numerous and careful dissections in support of his views, sufficient, as the question at present stands, to counterbalance their novelty, if not absolutely to establish their correctness.

On Gout; its History, its Causes, and its Cure. By WILLIAM GAIRDNER, M. D. Fourth edition. London: Churchill, 1860. 8vo, pp. 630.

WE have, after a conscientious sifting of the most reliable data, arrived at the very impartial conclusion, that Gout *en grand*, Gout rheumatic, Gout aponeurotic, Gout muscular, and all the other and several Gouts, have collectively consumed as much energy and thought as the "Origin of Evil," the Thames Tunnel, and the Greek Particle, all put together. Not that this outlay of labour has been followed by results at all commensurate, or by advantages comparable in any way with those accruing to mankind from the above-mentioned memorable evidences of human enterprise. The Greek Particle has made many a bishop; it is possible to get through the Tunnel, when you have found it; and the search after the Origin of Evil has led many a sinner to consider of his ways. But what can we say for Gout? There is a book just published, fresh from the pen of Dr. Garrod; here is another, by Dr. Gairdner; and in no one thing do these two gentlemen agree! A patient of our's, who has worn himself to a shadow, and spent "heaps" in mines, came to us joyfully, the other day, with good news. "Sir," said we, "what is it?" "Doctor," he replied, "we have received a report from the Tydvil-Morgans, and they have hit on a lode *congenial to copper!*" *We* don't hit on a lode congenial to copper. We work the mine to death, but, in the language of the money market, there is nothing for the adventurers. Gout is, in fact, the pyramid of medicine. There are a great many labourers: a large mass of rubbish: and nothing inside but blighted hopes and vain phantoms of broughams in the air.

Is it a painful or a gratifying fact to dwell upon, that any medical bookseller can readily supply more than three hundred works on gout? Is Great Britain gouty to a man, or are publishers the most sanguine of human beings? Are medical men related to these gentlemen by the mother's side, or are books written for wagers? Do M. Ds. feel irresistibly impelled

to treat of chalk-stones, as geologists do to knock them about with hammers? or is there something superlatively attractive to the mind freshly imbued with the truths of pathological lore, in deposits of urea?

These are momentous questions. To authors, doubtless, publicity is its own reward. "On the occurrence of an attack of gout," reads a gentleman in the writer's study, a night or two back, "severe pain is felt in the articulations of the fingers and toes." "Articulations! Dear me,-I never knew the toes could articulate." After that, none of us need despair of attaining to a high place in the estimation of the gouty ones.

What have we to show, then, for all this brain-work? Are we right in affirming a *materies morbi?* Can we analyze it—determine the conditions necessary for its development—the laws of its deposition—the means of antagonising it? Is our treatment less empirical than in Mrs. Steevens' time? "I have come to the conclusion," said an eminent author to us, "that there's nothing like Blair's pills. It's such stuff, you know. I go to a doctor, and he begins to muddle me. I ask him if colchicum won't do, and he dilates on the evils of quack medicine; and then the end of it all is, that he prescribes colchicum, and that's what Blair's pills are made of." Is this satire far from the truth? and have we nothing more to show for our work than the acetous extract, the wine of the seeds, and the bicarbonate of potash?

Not that *in limine* we wish to deal harshly with the work, whose title graces the head of this article. We must bear in mind that we are in presence of a great fact—a fourth edition —and, indeed, in a former Number of this Journal, we have, and with pleasure, bestowed our approval upon Dr. Gairdner's labours in the well-trodden field he is cultivating, with, apparently, such success. It is pleasant to find originality, where there is generally such sameness of matter and style; and safe rules for treatment, where mostly one is dazzled by hypothesis—even though the originality may be somewhat self-sufficient, and the treatment distinguished by nothing particular.

The motives that originally impelled Dr. Gairdner to write this work on gout are, we observe, persistent in his mind at the present time. It is but just that they be recorded, in order that posterity may not be altogether unmindful of the doctor's claims on its gratitude. After lamenting that certain observations and thoughts of his own are not reflected in the minds of his "colleagues," and recollecting that his own opinions have

undergone great change, Dr. Gairdner is driven to further inquiry; and, despite the diffidence which is the invariable attendant upon true merit, resolves to promulgate his opinions:—

"I shall be more readily excused for calling upon physicians to read another book, if the opinion I entertain of the great frequency of gout be correct. We are apt not to consider a man as gouty, unless he has suffered under a regular fit of the disease."

We are in duty bound to deprecate this display of ruthless jocularity in a scientific work. The author is evidently twitting his "colleagues" in London, who, it is notorious, are utterly benighted, and, on the question of gout, altogether in a sad way. We can, however, safely speak up for the practitioners in Dublin, and are confident there is not one of them who is not thoroughly alive to the frequency of gout as a latent source of disease—to whom gout would not suggest itself in any case of dropsy, or of iritis, or of fluid in the pericardium, or of cerebral softening, angina, palsy, or cardiac dilatation—signs, in fact, pertaining to diseases constituting, in our experience, the staple of consulting practice.

Agreeing, then, with Dr. Gairdner as to the frequency of gout, we naturally take stock of the prevalent notions on its history and nature, and inquire wherein the views of the author are notably at variance with them.

If we were asked to name a disease essentially constitutional, both in origin and progress, or a disease having for nature a depraved condition of the nutrient fluids, we should unhesitatingly specify gout. That the blood is the seat of gout, is the prevalent opinion; and so strongly do we incline to it, that we go even further in our belief, that a correct and thorough knowledge of this disease is at the bottom of all sound pathology. Nor do its manifestations, essentially local as they are, invalidate this view; for they are invariably accompanied by indications of constitutional disorder. The constitutional disorder may be idiopathic, or induced; the first preceding deposition, either in joint or texture; the latter, consequent on such deposition. The primary disorder originates in depraved assimilation, having its seat in the secreting or excreting apparatus; by which either the nutrient gland-cells fail in abstracting from the blood the elements necessary for their own nutrition; or the excretory gland-cells perform imperfectly their office of separating certain complex substances from the blood, therein deposited as effete material. How far this paralysis of function may depend upon disturbance in the nerve-apparatus of organic life;—or whether, if such there be, it may not itself

be dependent on abnormal nutrition, it is, and always will be, impossible to decide. The first certain fact, however, is a failure or perversion of the assimilative functions. A disturbance ensues as a *necessity* in the chemical balance of organic life, resulting in changes altogether incompatible with conditions of healthy vitality. The state of the system, we think, may aptly be compared to what is manifest to the eye in the occurrence of a chemical decomposition. A fluid, perfectly devoid of colour, is brought into contact with a re-agent, and forthwith, however minute the quantity of this latter may be, a change is induced in the whole mass of fluid. Its composition, the arrangement of its atoms, is altogether modified. Its nature, its physical characters, its chemical properties, cease to be the same. Before the gouty diathesis, however, is fully established, nutrition is not altogether stayed, and excretion is sufficiently complementary. But the poison is cumulative, becoming more and more stored up by its own impairing influence on the functions. When in sufficient quantity, a true toxemia is induced. This toxemia is, to say the least, most intimately associated with the presence of urea, uric acid, and its salts. Whether the poison or poisons be attendant upon these compounds, or be the compounds themselves, the first two accumulate in the blood; and one of these latter, the urate of soda, eventually deposits in the tissues. Further, we know that gout is frequently traceable to the parent immediately, and that it instances the curious phenomenon of atavism; that it may be engendered by a *vie oisive* and free consumption of aliments rich in carbon and nitrogen, and averted, or at least mitigated, by abstinence and exercise.

We think there is no room for doubt but that the humero-vital doctrines are alone capable of reconciling these several phenomena. Can there be anything more analogous to the zymosis of a miasm and the formation of an eliminative pustule in an eruptive fever than (although not by fermentation) the accumulation of urea and the deposition of the soda salt? It is as well, therefore, that we should benefit by the matured views of Dr. Gairdner, as expounded in the present edition, and these are they:—

"Venous congestion, then, I consider the first condition essential to the formation of the gouty diathesis. It is no new observation; it may be found interspersed through the writings of all former authors. Even those who adopt explanations inconsistent with such a state of things, notwithstanding, admit it. This state of the blood was first clearly announced as the great cause of gout by Galen, whose opinions have continued to influence the minds of suc-

ceeding physicians, in a greater or less degree, to the present day. The truth of the fact being, I imagine, unquestionable, it will always continue to embarrass the doctrines of those who advocate opinions with which it is incompatible.

"But the great venous canals of the body, as well as the larger arterial vessels, are endowed with a resiliency which enables them to struggle well against the flood of returning blood. This fluid, then, is compressed between two opposing forces, that, namely, which is derived from the heart and arterial system, urging it forward on its course, and, on the other hand, the antagonistic resistance of the great veins leading to the right auricle. Under this compression I believe that the vessels give way, and a true hemorrhage is occasioned in the part affected. If the rupture take place in a minute capillary carrying the serous portion of the blood only, œdema is the consequence; but if the burst vessel be one carrying red blood, a true ecchymosis is formed.

"This view of a fit of gout may startle, from its novelty; but I am thoroughly convinced, from long observation of the disease, that I have given the true rationale. All its symptoms may be readily arranged under this explanation. Any other that I have ever heard of leads us into such difficulties, as to leave us only in greater doubt than before. It will surely be admitted that the capillary and nutrient vessels, distributed on the extreme and sentient fibrillæ of the nerves, are affected in the same manner as the larger venous trunks. I believe these distended capillary vessels are the real seat and cause of the painful phenomena of gout."

It will be gathered from the above that venous congestion is essential to the production of the gouty diathesis and to the fit itself. The direct inference is, that slight venous congestion induces the diathesis; intense venous congestion, a paroxysm. Between the two conditions are many intermediate states, readily accounting, it would seem, for the phenomena of this disease;—its hereditary character, its metastasis, its association with peculiar states of the urine, its affection for the joints. If the gouty diathesis is dependent on venous congestion, what does the venous congestion depend on? Certain sects, once on a time, affirmed that the world rested on an elephant, who was supported on a willing tortoise. "But," said the captious of the period, "what does the tortoise stand on?" We would delicately suggest that the faulty elimination, deduced as a consequence of the fit, is the cause of it. To affirm that a paroxysm of gout is due to the hyperemic condition of the viscera, is to reiterate the exploded view of Dr. Parry, to whose patients Dr. Prince recommended the tread-mill, and who presumed, that not only gout, but most diseases, consist in local determinations of blood.

Venous congestion must, in truth, itself be a result for which true physiology would at once seek for a cause. Arrest of the circulation in the veins must be owing to one or more of these influences—deficient resistance of vein-coat, want of power in the heart, obstruction between congested spot and the right heart. Now, in order to establish the gouty diathesis, is one of these causes indispensable? We think not. Indeed, we have but to open the head of a person dead with gout in the brain, and palsy consequent upon the establishment of the gouty diathesis, to find the utter absence of venous congestion, and the presence of white softening. Indeed, the ganglionic masses are often found in a state of semi-deliquescence from interstitial atrophy, a condition demonstrative of a nearly total deprivation of blood from the cerebral pulp; a state of the system opposed to plethora, and a condition of the brain altogether different from that observed in sanguineous apoplexy, where a laceration of brain-fibres ensues as a consequence of venous congestion. On this congestion-theory, gout should reasonably be expected to develop itself preferentially in those structures, and organs exhibiting them, which are dependent for their integrity upon that of a very complex venous system. Such, for example, are the liver and the kidney; but we are not aware that obstruction in their portal systems results in gout, or that this disease manifests any predilection for either of these glands.

Is, then, this supposed venous congestion due to want of resilience in the vein-coat? Such deficiency must be ascribed to a withrawal or impairment of the nerve force to which the coat itself owes its physico-vital properties. And, similarly, where the heart itself is deficient in power, the *origo mali* must, in like manner, have its seat in the nerve-centre or in the conducting element. If, again, we assume an obstruction between the seat of gout and the right heart, such obstruction—its equivalent, venous congestion—cannot but be regarded as itself dependent on an exciting cause.

In truth, venous congestion is only one of the signs of that condition of the body of which gout is the exponent. Venous congestion occurs in a vast number of diseases and states of the system; at hap-hazard, for example, in inflammation: in purpura: in bronchitis: in cerebral hemorrhage: in pregnancy: in varix: in gangrene. Frequently these conditions, or some of them, may be immediately deduced from venous congestion. It would be far more philosophical to conclude this venous congestion to be *the* cause of these several diseases, than that it is the cause of the gouty diathesis, for in this latter it often happens there is an utter absence of congestion of any kind.

What, now, presses in favour of the humoral view of gout? *A priori*, it is as reasonable to suppose a *materies*, as a *defectio* or an *obturamentum;* a special poison, acting on the whole constitution—on all the systems at once—as a faulty state of the venous system always preceding hyperemia. A poison admits the phenomenon of metastasis; not so venous congestion. A poison, syphilis, for example, can be transmitted from the parent. Can venous congestion? A poison may be eliminated; and, if a poison there be, its effects may be mitigated by the elimination. No such evidence testifies in favour of venous congestion. Why can a gouty stomach bear a pint of brandy? Surely not because it is congested. It is a fair hypothesis to allege that the mucous membrane of the pharynx and the general cutaneous surface, throw off a poison in scarlet fever. It is sound induction, when we find the urine albuminous, and the kidney inflamed, after scarlet fever, to affirm that this organ has been endeavouring to throw off this poison, and has been damaged in the attempt. But we cannot get at the poison, nor do we know anything of its real nature. Yet it is fair to assume its existence. How much more, then, are we not justified in believing gout to be due to a poison, when in this disease we not only observe similar facts warranting similar inductions, but are able to get at the poison itself; to isolate it; to note its invariable chemical identity, and its constant attendance upon the disease; to arrest it in the circulating fluid, and at any point of the circulation; to mark its deposition as a vital precipitate, wherever and whenever the juices are subject to its influence?

From what has preceded, our readers will not be surprised to hear that Dr. Gairdner denies altogether the existence of an uric acid diathesis. Noticing the discovery of Dr. Garrod, that uric acid is found in the blood; that it had previously been detected in the joints by Wollaston; and by Landerer as a concretion between the coats of the aorta; Dr. Gairdner goes on to say:—

"One of the most remarkable results of arthritic indigestion is the presence of acid in nearly all the excretions of the body. This has of late years been called the uric-acid diathesis. The adoption of such a term would imply a conclusion to which I am by no means prepared to assent. By a diathesis, I understand a condition of the constitution, and not a simple affection of certain fluids of the body. Undoubtedly this acid condition is very general.

"But, admitting that these accumulated facts are sufficient to justify us in the use of the expression to which I have adverted, it could not, by any means, be called the uric-acid diathesis, for it ap-

pears certain that various acids make their appearance in the secretions. Berthollet supposed that the acid of the sweat was the phosphoric; Berzelius and Anselmino showed it to be the lactic; Thenard found acetic. No one, too, as far as I know, has ever found uric acid in the stomach; but there seems little doubt of the presence of the hydrochloric and lactic, and the acetic, in cases of dyspepsia. This idea of a uric-acid diathesis as the cause of gout, meets with another and a fatal difficulty, from the fact that urates in the urine are common to gout with many other diseases. Ordinary indigestion, from whatever cause, or a slight and ephemeral fever, will cause this appearance. Dr. Prout has indeed here established a solid and useful distinction between that deposit of urates which is occasional and transient, and that which is more durable. 'The lithate of ammonia in the urine is one of the most common attendants of slight dyspepsia from errors of diet.' But the same substance makes its appearance in the urine in a less transient manner, in the course of severe diseases not of a local nature, 'when no food has been taken into the stomach, and when, therefore, its formation can only be attributed to secondary mal-assimilation of the albuminous contents of the blood, and albuminous tissues.' I have seen, too, and I am sure Dr. Prout has seen, very durable deposits of the urate of ammonia in cases, such as lepra or psoriasis, where no great constitutional disturbance existed, where no unusual disintegration of the tissues was going forward, and where no interruption to ordinary wholesome nourishment was suspected. But, in order to establish the presence of uric acid in the circulating fluids as the cause of gout, it would be necessary to prove that it is never absent."

If we apprehend the author correctly, he objects to the term diathesis, because the uric is not the only concomitant acid of that state of body of which it is characteristic. That is to say, a person whose urine deposits lithic acid and its salts both in excess, and during considerable and frequently-recurring periods, manifests no lithic-acid diathesis, for the reason that other acids are simultaneously excreted by the emunctories; phosphoric, by the skin-glands; muriatic and acetic, by the gastric follicles. Surely, this is no more than to say, that a disturbance in the balance of the excretory system is impossible, as long as any one of the minor functions is properly performed. Supposing phosphoric acid were an abnormal product, or were produced in abnormal quantities, or, so to speak, at a wrong point of the system, we should not hesitate to believe in the phosphatic diathesis. By diathesis we suppose a tendency, and when the tendency is confirmed, we call the result a cachexia. In the hemorrhagic diathesis, there is a tendency to bleeding; in the oxalic diathesis, a tendency to the formation

of insoluble salts of the acid; and such diathesis may be temporary, or irremediable, and, as it were, an element in the vital equation. We see, to the belief in an uric-acid diathesis, none of the "fatal difficulty" that oppresses Dr. Gairdner. Unquestionably, urates in the urine are common to many other diseases than gout; but a diathesis is not negatived by the manifestation of its *characteristic* incidentally in the course of other diseases, not resulting from that diathesis. For example, it is by no means unusual to detect sugar in the urine after eating starch, or in the urine of patients attacked by pneumonia, or by pharyngitic epidemics with exudations of plastic lymph. Is it, therefore, to be inferred that a diabetic patient presents no saccharine diathesis, because he eats starch one day, or has a solidified lung, or diphtheria, on the next? Everyone believes in a tubercular diathesis. But, according to this view, a patient dying of phthisis pulmonalis would not present it, should tubercles deposit in the omentum—nor another, the hemorrhagic diathesis in purpura, for the reason that petechiæ form in continued fever.

The fact seems to be that, in all cases, long previous to an attack of gout, the system is in a state of active waste. The result of this waste is uric acid and its compounds, the decay affecting the fibrinous and azotised tissues generally; or that, previous to conversion into texture, the nutritive element is thrown out without subserving nutrition and repair. However this may be—whether the nutritive matters be supplied in excess, or the selecting power of the textures themselves be deficient—a *degrading impetus* is sustained, and organised material is converted into uric acid. The discovery of Dr. Garrod enables us to conclude that the acid in question is not formed by the kidney, but in the laboratory of the system at large. How long the casting off of this poison may go on, it is, of course, impossible to say. The limit to its elimination is dependent, no doubt, on the inherent powers of resistance to its influence which pertain to the individual, and to the ability of the several glands to cast it off. In some persons, profuse acid secretions of the skin—in others, copious deposits in the urine, indicate the attempt to throw off the burden, and for a time the effort is successful. But by degrees, from contamination of the nerve apparatus with the poison, the innervation of the glands is modified, and partially annulled. Antagonism then ceases; and deposition forthwith takes place in some portion of the system, offering the least resistance to its action. As a rule, the joints are the most attacked; but they are not the only localities selected. Dr. Garrod goes so far as

to assign to inflammation the power of inducing the deposit at the inflamed spot; but herein he seems to refuse the very advantages the humoral view bestows of educing the complete history of a paroxysm; and to our thinking it is the truest, as well as the most reasonable view, to look upon all inflammation as a consequence rather than a cause.

The uric-acid diathesis we do not intend to assert is peculiar to gout, but it is more peculiar to gout than to any other known disease. We agree with Dr. Gairdner that many states of the system are characterized by its presence. Where such is the case, gout, we should infer, was on the road; and where gout itself is present, we believe the uric-acid diathesis to have gone before, and to have been essential to it. If this be true, an attack will be best averted, and a paroxysm mitigated, by so influencing the nervous system that it may, by resistance to the influence of the poison, be sustained in its attempts to further the eliminative process.

We have been led to this conclusion by frequent observation, in common with Dr. Gairdner, and both in hospital and private practice, of the simultaneous deposition of phosphates and urates, and the alternation of the two sediments. We have a shrewd suspicion one deposit is to a great extent vicarious of the other; and that the causes bringing about the degradation of the azotised principles have a like influence on the tissues abounding in phosphates. It would be interesting to determine the exact relation these two deposits bear the one to the other. Does a deposit of urates invariably precede one of phosphates? or does the waste of nerve texture, denoted by the latter, bring about the excess of urates, and become in fact, the cause of that deficient vital resistance ending in the waste we above have alluded to?

Endeavouring to demolish the uric-acid hypothesis, Dr. Gairdner calls for an explanation of the fact, with which he believes it irreconcilable, "that the first attacks of gout, when the so-called uric-acid diathesis is least developed, are generally by far the most painful," and then continues:—

"But the inadequacy of Dr. Garrod's hypothesis for the explanation of the attacks of regular gout becomes hopeless deficiency, when we advert to the phenomena of the atonic disease. Could I admit his dictum, that an excess of urates may lurk in the blood without notice of their presence, my difficulty might be less, though by no means removed. But I entirely repudiate this unfounded opinion. It is offered to me without proof, in the shape of an assertion. I see in it only an assumption necessary to complete an erroneous theory. I am quite satisfied, from long and *painful* ob-

servation, that the presence of an excess of urates in the blood is always attended with more or less distress; that the great majority of cases of suppressed atonic gout present not a trace, not a sign of uric-acid diathesis, either in the urine or the blood; that this makes the great difficulty of their treatment, and that their cure is coincident with the appearance of this supposed cause."

The explanation of the extremely painful character of first attacks of gout, we think, is easily arrived at. Pain is the result of a morbid impression on sensitive nerves, which eventually become inured to the poison. The unnatural stimulus acting upon them, the nerve-tube and its contents undergo an actual chemical change. The nerve is partially disintegrated, and perfect *polarization* takes place no more. After successive vitiations of the nerve-texture, there is no longer that violation of those previous and integral conditions, the primitive disturbance in which manifests itself in pain of an acuter kind than that which results at a later period.

That this view is correct, we think, is demonstrated by the gradual softening and degeneration of nerve-substance, whether in fibre or in ganglion, that always ensues upon the long persistence of gout. It may be urged that gouty persons do, on the contrary, possess perceptions as clear, and attributes of nerve-power as subtle, as those of other men. But such an objection would originate in the omission to take into consideration the portion of the brain-mass affected; and to invalidate the above view, it would be necessary to show either that the poison settles by preference upon the hemispheres, or that the hemispheres were of collectively greater mass than the centres of motion, of sensation, and of co-ordinating power.

We are not inclined to go to the same length in condemnation of Dr. Garrod's hypothesis as Dr. Gairdner. We see no violation of analogy in supposing that urates may lurk in the system. Will Dr. Gairdner maintain that the agents producing diseases not gouty may not remain latent, giving no outward and visible sign? We would, for example, instance ague and epilepsy, not to speak of the dormant state of the cancer-cell, and the incubation-fever periods. In ague at regular, and in epilepsy at irregular intervals, a manifestation occurs of the presence of a disease which, meanwhile, might be supposed to have no existence. Are the diseases and the elements of them absent between the fits? present only when recognizable by the senses? Is the patient suffering from ague only when he is shivering, and another afflicted with epilepsy only when foaming at the mouth? The facts point the other way. The diseases, perhaps the causes producing them, cer-

tainly the agencies perpetuating them, are present, though all signs and symptoms are absent. The body is epileptic during the interval between the cessation of a fit and the advent of the next; and so, in like manner, is it suffering from ague, though the shivers are not "on." And, in fact, Dr. Gairdner allows this to be so; for in that portion of his work which has the most originality—that, namely, where he classifies gout—he affirms the persistence of the disease during the intervals between the attacks.

Cullen recognizes four so-called species—varieties they would be better termed—of gout. They are the varieties recognized by Good, and admitted by Scudamore and Macintosh. Such are the regular, the atonic, the misplaced, and the retrocedent. Of these, Dr. Gairdner admits only the regular and the atonic; and, in describing the regular variety, divides it into stages, each of which may be marked by successive paroxysms or fits. This is, in fact, to acknowledge that the gout is present in the intervals, when there is an absence of all deposition or inflammatory action, and when no trace of gout is to be observed.

The author, in dilating on his own classification, becomes graphic, and revels in symptoms, as Abernethy did in a poultice. But the symptoms will hardly be a source of misinterpretation to any physician of the present day. They are simply those that may be educed by a consideration of the tissues affected, and the perversion of those functions to the performance of which they are subservient.

"The earliest sign of an approaching fit of the gout, to which my attention has been drawn, has been a dull pain in the left side of the chest, accompanied by an inability to lie on that side, and sometimes by fluttering, irregularity, or intermission in the action of the heart. These symptoms often continue for a great length of time, without any perceptible increase. Patients not apt to take alarm about themselves, frequently suffer them in silence, and even forget them."

This subsidence or "sinking" in the chest is a frequent premonitory symptom, accompanied by intermittent pulse. In our experience, however, most patients anticipate an attack from disturbance connected with the digestive organs. We have a patient under treatment, at this moment, in whom an attack is always preceded by distressing flatulence. Three or four days after the advent of this symptom, the gout "flies to one of the joints." As soon as the inflammatory acme is attained, all gastric symptoms disappear; but while the dys-

pepsia lasts, our friend is obliged to lock himself up, as the constant noise resembles that of a park of artillery announcing the coming of a conqueror. This patient has never presented a symptom indicative of functional cardiac disturbance.

The following accords with general experience:—

"The local manifestations in the joints are usually looked to as the most characteristic signs of impending gout.

"Yet, certainly, persons who are threatened with a fit of gout, do very often experience great tenderness of the feet in walking, weakness and pain of the ankles and wrists, pain of the ischia in sitting, and frequently have swellings of the articulations of the fingers and toes."

We have in our mind a gentleman in whom the joints are exquisitely susceptible. An attack of gout may at any time be brought on in this unfortunate patient, by treading on his toes, or squeezing a finger. Experience has taught him some severe lessons in this particular; and he rigidly taboos all shaking of hands, and other such like demonstrations of friendship. When he sees any one about to shake hands with him, he puts his arm out at full length, and points the little finger. If he were not known as the goutiest, he would be certainly thought the oddest of men.

One anomalous symptom referred to by Dr. Gairdner is pain in the tonsils (p. 11). This, we should think, is very rare, nor have we ever met with such a case; but, on the other hand, many attacks of gout we have found declaring themselves by giddiness, and hesitation of speech. It is a question whether the several symptoms may not each be taken more or less to indicate the kind of gout that is coming on. Thus dyspepsia, irritability of the bladder, &c., we think, may be looked upon as denoting that the attack will expend itself mainly upon the mucous membranes; tenderness in the joints, that the synovial and serous membranes will be principally visited; pain in the heart, and irregular action of it, that the muscular system will be most punished. We should have been glad to note the results of Dr. Gairdner's observations upon the renal secretion, for we are satisfied much remains to be done in this department of physical medicine. The urine, our author justly observes, is mostly, at the onset, scanty and high-coloured; and, at the close of the attack, throws down a deposit, which, usually supposed to be urates, may consist in great portion of mixed phosphates. Not unfrequently, however, we have seen the urine voided in large quantities. In many patients the symptoms are, it is true, those of pyrexia; but, in some others, it

would seem as though a temporary hemotrophy of the kidney were brought about by the increased efforts at elimination. Not unfrequently, indeed, the urine is diabetic, with sp. gr. higher than 1020, a condition rarely met with unaccompanied by diuresis. We do not know why the presence of oxalate of lime, and the alternation of that salt with lithates, are not alluded to in the work, or why no information should have been afforded on the very frequent excess and probable nature of those obscure principles of the urine, which display themselves by the modifications in colour produced by the addition of nitric acid.

That many, if not most, of the terminations of gout are to be referred to annihilation of function in the kidney, is generally conceded at the present day, when so much light has been thrown upon the pathology of this organ. We should have expected that a fourth edition of a work on gout, published at this time, would have noticed the views and researches of Frerichs, of Todd, and of George Johnson. What would be Dr. Gairdner's prognosis with an abundance of oil-casts? Is the microscope of any practical value in cases where the diagnosis is uncertain? Is there any special nephritic affection attendant upon gout? Does the elimination, set up mainly through the kidney, induce any peculiar changes in the uriniferous tubes and their lining epithelium? These are, perhaps, the most important questions connected with practical physic; and, in a fifth edition, we trust some notice will be taken of the advance that has been made in this department of pathology, since the publication of Dr. Johnson's celebrated paper in the Medico-Chirurgical Transactions.

We are inclined to believe that that peculiar modification of chronic nephritis, or, to speak more correctly, the contracted granular kidney, may not only be the invariable concomitant of gout, but be, at times, the only result of the diathesis. We do not doubt but that those morbid atrophic lesions, characteristic of the influence of gout, are to be observed even more frequently than those changes effected by the *materies morbi* in the joints, the sheaths of tendons, the bursæ, and the synovial membranes. Most kidneys of persons suffering from gout would, we think, could they be examined during life, present traces of the shrivelled granular appearance. Waste is the rule in gout. In the kidney, this waste takes place at the expense of the secreting structure—the cortical portion presenting, on a cut section, the granular appearance identical with that of the surface, from which the capsule may be torn with great facility. The microscopic appearances are, under these circumstances,

peculiar. It is common to find the tubes altogether free from epithelium. Others of these tubes are semi-denuded, shrivelled, and folded; and not unfrequently a degeneration of the epithelial cells into fat is found to have occurred. Simultaneously with these conditions, an obliteration of many of the blood-vessels takes place—a state of things accounting for the wasting of the organ. It is by no means unusual for tubes to disappear altogether, and to be replaced by cysts, a phenomenon accounted for by Dr. Johnson, by the assumption that the tubes may become altogether transparent from the loss of their epithelial lining.

How may these pathological changes be made subservient to diagnosis? A patient about the middle period of life is, say, free from dropsy. There are no pathognomonic signs of gout. There may be no hereditary taint, nor even a history of sedentary life, or free indulgence in "alcoholic food;" nor is there any disturbance in cardiac function. But if the urine be allowed to stand for a few hours, the secret comes out. Examine the sediment. There are no lithates, no uncombined lithic acid; but there is an amorphous substance—the disintegrated epithelium of the kidney-tubes—part of which is aggregated together in the form of tubes, and called by Johnson, granular casts. With this condition, there may be very little or no albumen. The quantity of the urine may be normal—certainly not diminished—and its sp. gr. even remarkably low.

Such a case, therefore, as that narrated at pp. 80–84, is to the physician of little value. The case is one of atonic gout. During the progress of the disease, partial dropsy comes on. The urine is tested several times, but presents no evidence of albumen. The *post-mortem* examination expends itself mainly upon the circulatory apparatus; and the kidneys are thus dismissed, "the kidneys were healthy." We doubt that the kidneys were healthy, and should have suspected them to be in a state of granular degeneration, even without the dropsy. The absence of albumen is often noticeable. One day the urine may be albuminous; the next, free from all traces of the principle. In the last stage of granular kidney, we have had frequent occasion to observe an utter absence of albumen; and, under similar circumstances, we may safely conclude that the atrophy of the kidney is very far advanced.

Dr. Gairdner's treatment is, we hope from the success that has attended it, decidedly antiquated. We see no mention of applications of spirit of wine, or the oil of horse-chesnuts, to the joints. Alkalies and earthy carbonates, as might be expected from Dr. Gairdner's denial of the uric-acid diathesis, are

pronounced "wholly inefficacious," p. 323; and the carbonate of lithia is not exempted from this disparagement. We humbly trust that such an authority will by-and-by see good reason, at all events for abstaining from those blood-lettings which at present have his cordial approval; and will believe that the success of a work need not be diminished by its author posting himself up in the science of his subject, even though the knowledge thus added to the text should necessitate the curtailing of some very scholarly and agreeable writing.

On the Signs and Diseases of Pregnancy. By THOMAS TANNER, M. D., F. L. S., &c. London: Renshaw. 1860. 8vo. pp. 504.

DOCTOR TANNER does not come before the profession for the first time. He has produced a variety of medical works, some of which have passed through more than one edition. Thus, there are his "Practice of Medicine," his "Treatise on Diseases of Infancy and Childhood," his "Manual of Clinical Medicine," his "Memoranda on Poisons," and his "Select Medical Formulæ."

The Doctor must have commenced his literary career previous to 1853, inasmuch as the *second* edition of "Select Medical Formulæ" *reprinted* from his "Manual of the Practice of Medicine," bears the date of 1854. The volume of "Practice of Medicine"—we have not the original edition by us—may, therefore, be presumed to have made its first appearance in 1852 or 1853, perhaps earlier. Now, as we are informed in the Preface of the volume before us, that its author commenced practice "some thirteen years ago," or in 1847, it follows that his first, and, according to its title, we presume his chief work, was given to the world at the expiration of the fifth or sixth year of his practice. Strange as this may appear, still more strange is the fact, that during the period when he was gathering materials from his practical experience, to write a *practical* treatise upon such an important branch of our profession as *practice* of medicine, our author tells us his "horæ subsecivæ were not altogether few or far between." So that the absence of patients, by no means an uncommon state of affairs in the first portion of a physician's career, was no bar whatever to the production of a volume, teaching us how to discover their ailments, and to assist nature for their removal!

Not satisfied with having produced all the publications above enumerated, from time to time, in the latter half of a

period of thirteen years, Dr. Tanner now presents us with an *opus magnum.* This our readers may presume to contain the result of the full period of, at least, six years' experience, when it is hoped his "horæ subsecivæ" were not so numerous. And here we may observe that the "horæ," which he devoted to the revisal of the proofs of the volume *now* under consideration, might probably, with more propriety, be termed "subsecivæ" than those which he spent—not having anything else to do—perusing the writings of others; since the proper signification of "subsecivus" is, stolen, spared, or borrowed from *other business.*

Be this as it may, our author grew a

"Midnight student o'er the dreams of sages."

He conceived he could not occupy his spare-time better than by consulting "many dusty old volumes," with a view towards mental exercise; and having stored his mind with the ideas "of those great ones of the past, who can now only speak to us by their writings," he here acts as the medium of communication between the dead and the living.

Taking, therefore, all the circumstances above set forth into consideration, it must be confessed, we are not in the least surprised at the author's presentiment, that "the references which are made to old authors (he might, also, have added modern) may, perhaps, appear more numerous than is desirable."

This is truly the age of book-making; and amongst no class is the *cacoethes scribendi* a more prevailing epidemic than amongst ourselves. We were fully conscious that if an old doctor, now-a-days, spends his "horæ subsecivæ" at ever so quiet a watering-place, he expects us, subsequently, to peruse his "physician's holiday;" but we never contemplated the probability of our having to wade through a set of annotations, collected during the course of elementary study to which a young physician submitted himself, for the purpose of acquiring sufficient knowledge to enable him to *commence* the business of his profession.

Such is the nature of the volume before us—a compilation from beginning to end, with here and there a few running commentaries of the author's own, as to whether his experience coincides, or the contrary, with the experiences set down. In other words, this book may be looked upon in the light of a "grinder's" note-book, relative to the signs and diseases of pregnancy; and, in this respect, it partakes of the character of all Dr. Tanner's publications. Therefore, in taking a dip here

and there through its pages, we shall observe, not so much upon the author's own experience as upon that of others.

The great advantages of auscultation in obstetric medicine are now placed beyond controversy; but there was a period when its practice was scouted by more than one eminent member of the profession. Thus the author mentions Hamilton, with whose views on this subject we are all acquainted; also Lyall, who, in 1826, wrote as follows:—

"Of the utility or uselessness of auscultation in discovering pregnancy, we have had no experience. It is said that the operation may be performed either by applying the ear to the different parts of the abdomen, or by using the stethoscope of Laennec. Reasoning *a priori*, we anticipate little advantage from such examination."

This language, written in 1826, does not excite our surprise, and may be excused; but it *is* a subject of wonder that in the present day, notwithstanding the overpowering testimony of, we may say, all the profession, an individual can be found, who gives utterance, not only to ideas similar to the above, but whose convictions on the use of the stethoscope to determine pregnancy (if we may call them convictions), he has set forth in the following manner:—

"Success in detecting the fœtal heart depends, in a great measure, upon circumstances; whether or not the auscultator considers that *he ought* to detect them."

And he continues:—

"The whole system of fœtal ascultation originated soon after the dawn of general auscultation, when men's minds were excited by the love of novelty, and warped by many erroneous impressions and mistaken modes of thinking, and has since been mainly upheld by authority."

However excusable such language might have been in the days of Lyall and Hamilton, it is now unpardonable. "I cannot but think," the author observes, in allusion to this gentleman's writings, "his papers would never have been written, had he only taken the trouble to visit the wards of our lying-in hospitals before sitting down to his desk." We say, however, that to this individual should be applied the gross charge that he has applied to others, viz., "that it depends in a great measure upon circumstances, whether or not the auscultator considers that *he ought* to detect" the sounds of the fœtal heart. And that the mendacity (for it is nothing less), which he lays

at the doors of others, he would be guilty of himself, by pretending he *did not* hear sounds which *he did* hear. Even supposing Dr. Francis Adams, from some defect of his auditory apparatus, is rendered incapable of detecting the sounds of the fœtal heart, surely he is not warranted, on that account, to ignore the assertions of those who say they do hear them. However remarkable Dr. Francis Adams, "the learned translator of the works of Paulus Ægineta," may be for his classical learning; we cannot, after this, acknowledge him as a practical physician.

Dr. F. Adams' opinions appeared, it seems, in some Numbers of the Medical Times and Gazette, during 1859–60—opinions which must have created some surprise, and no little amusement. Strange to say, while engaged on the present observations, we have before us an article in the same Journal of as recent a date as the 5th of the past month, which bears upon the subject in question. This novel article would afford us considerable mirth, but for the regret that one professing to be of our calling should give utterance to such trash, exhibiting his ignorance and folly: and that a respectable Journal should prostitute its pages by the admission of such garbage. The thing is signed T. C. D.; but, for the honour of our Univesity, we trust the initials do not signify Trinity College, Dublin. The following is a copy of the document, which we think worthy to be placed in antagonism with Dr. Adams' opinions on obstetric auscultation:—

"I ponder'd by the bed-side,
I walked about the room;
The stillness of my patient
Fill'd my mind with deepest gloom.
The os was firm and rigid,
Not a fibre of it stirr'd;
And the beating of my own heart
Was all the sound I heard.

"I sat me by her *right side,*
And watch'd her pale, pale cheek;
But as it grew still paler,
I did not dare to speak.
I watch'd her for a moment,
Then asked her for a word;
But the beating of my own heart
Was all the sound I heard.

"It came not; no, it came not;
The night was passing on,
I knew not but my patient,
And her fœtus might be gone.
To the precept of my teacher
A *happy thought* referr'd,
When he told me most profoundly
What sounds were often heard.

"The stethoscope—then gently
I press'd it to her side,
As though *she'd been an angel,*
Or were to be my bride ! !
I press'd it nearer, nearer, nearer.
A sound! 'tis clearer, clearer, clearer.
I did not speak; no, I did not speak a word,
But *the beating of the fœtal heart it was the sound I heard.*"

Our only excuse for quoting the above is, that emanating, as it evidently does, from a person altogether unacquainted with obstetric medicine, it is, therefore, the more worthy of being brought forward to refute the opinions expressed by Dr. F. Adams, who is equally innocent of it.

Dr. Tanner mentions, parenthetically, the invaluable testimony afforded by auscultation during parturition,—in those tedious cases, for example, in which the fœtal heart can be heard to pulsate naturally during the early stage of labour, and, as the labour advances, to beat unnaturally; in other words, slower or quicker than previously. We fully concur as to the great value of auscultation during tedious parturition, as indicating the state of the child's circulation, and consequently the time for interference; but we cannot quite agree with him as to the differential diagnoses he thus forms in such cases. He says:—

"When in tedious and dificult labours we find the fœtal pulse becoming slower and slower, we may reasonably infer that *undue pressure is being exerted on the funis*, thereby causing the äeration of the blood by the placenta to be imperfectly performed. When, on the contrary, the beats of the heart increase in rapidity, and especially when they likewise become irregular and intermitting, it is probable that the danger does not result from the pressure upon the umbilical cord, but *from compression of the brain*, or *from a source causing cerebral irritation.*"

Thus, Dr. Tanner conceives the effects of undue pressure on the funis can, in a measure, be distinguished from that ex-

erted on the brain of the fœtus, by auscultation during parturition. In the former instance, the pulse becomes slower and slower; whereas, in the latter, from being accelerated, it becomes irregular or intermittent. We cannot go with him so far, even granting deficient aëration of the fœtal blood *alone* originated from compression of the funis. It has been our experience, that the fœtal pulsations become *very frequently* such as he describes them in the second instance (viz., from a state of acceleration to that of irregularity and intermission, as indicating, according to his views, the condition of cerebral compression), when there was evidently no undue pressure exercised on the cerebrum; but when, on the contrary, the origin of the change of the fœtal heart's action most indubitably arose from imperfect aëration. Impaction, more or less complete, or undue pressure on the cranium, can be recognised without the aid of the stethoscope to a certainty. His using the phrase, "or from a source of cerebral irritation," shows that his theory is not very sound, since the fœtal blood becoming impure *is* a source of cerebral irritation.

Moreover, we do not admit that in the majority of instances of tedious labour the imperfect aëration of blood arises from compression of the funis. It very frequently happens that the contractions of the uterus, in these cases, even though they be inefficient, are, to a certain extent, continuous, perfect relaxation never occurring. The fibres thus maintain a sufficient degree of permanent spasm to arrest the current of blood from the curling arteries through the great placental sinus, and thence onwards through the sinuses of the uterus.

This origin of deficient aëration has also, on many occasions, been exemplified to us in rapid deliveries. In such cases the uterine action was not only powerful, but the contractions were so rapid, as to be without any decided intervals of complete relaxation. We do not consider pressure on the funis so frequent a cause of injury to the fœtal circulation, as that to which we have just referred; indeed, we deem it more rare than suspected.

Our Continental brethren always aim at bearing the palm from us in acuteness of diagnosis; in the application of the stethoscope for the purpose of arriving at conclusions, they consider themselves peculiarly *au fait.* Here we are introduced to Doctor Frankenhauser, "who has been often able to say correctly, before birth, what was the sex of the child," by means of the stethoscope; and who, as the author informs us, "seems to infer that his rule *has never failed him,* provided it was put

to the test before the contraction of the uterus during labour had deranged the *natural frequency* of the fœtal heart."

It is from the difference of the average frequency of the heart's pulsation in the male, as compared with that of the female fœtus, that Frankenhauser draws his conclusions as to the sex of the unborn child, before labour has commenced. His theory is, that the mean number of beats of the fœtal heart are more frequent in the female than the male; the mean natural frequency being 144 in the former, and 120 in the latter. However, when we come to consider that the greatest number of beats in the female is given as 138, that the pulsations run as high as 132 in the male; and that we have every variety of number from the mean in both to the extreme in both, we are not surprised that the author's "experiments seem, at least, to prove that Dr. Frankenhauser's observations will not apply to the intra-uterine children of this (London) metropolis." Neither, say we, to our own visceral population.

We must also notice another Continental refinement in obstetric diagnosis. Not satisfied with the aids of sight, touch, and hearing, Dr. Pollender gives a constant and unerring method for determining pregnancy by means of his nose! According to this authority, the vaginal mucus, during the pregnant state, has a "musty" smell, "something like that of the spermatic fluid or liquor amnii, and after examination *it cannot be mistaken for any other odour*." It appears that Dr. Pollender never failed in a single instance to make a correct diagnosis by means of his olfactory test; and, "according to his experience, the odour is perceptible as early as the eighth day of gestation." *To this*, as a test of pregnancy, *if it be one* (for we have not, as yet, given our nose a *complete* obstetric education), we would apply the words Hamilton made use of with respect to auscultation, viz., "few cases can occur in actual practice where this test can be required." In the present state of our knowledge, it is quite unnecessary, and its publication is repugnant to our feelings. The idea to us, Islanders, of *smelling out pregnancy*, would be ludicrous, were it not revolting, so we shall allow Dr. Pollender to exercise his invention for his sole advantage. In a short time, it will not surprise us to hear that the sense of *taste* has been called into Continental requisition in obstetric diagnostics. It is unnecessary further to notice the contents of this volume.

As a note-book, it is well arranged, and gives fair evidence of the author's careful and copious reading.

Those who do not possess any of our valuable and comprehensive obstetric class-books, who have it not in their power to

consult Encyclopœdias, or the standard original monographs upon the signs and diseases of pregnancy, may perhaps derive some benefit from the purchase of Dr. Tanner's compilation. It remains for us to say, that such a description of book was not required at the present moment; and we repeat, that it partakes too much of the nature (as all Dr. Tanner's works do) of a "cram-book." Had the author informed us that it was so in his preface, we might have dealt more leniently with him; but after so high-sounding a title, and such a bombastic introduction, we had a right to expect something better at his hands.

In conclusion, we are constrained to observe, that for one who has not yet had time to mature his experience, the author speaks much too confidently on points of diagnosis, and upon the utility of medical agents. With Dr. Tanner's opportunities, however, combined with his great habits of industry, we have no doubt but that, at some future period, he shall present us with an original and *practical* volume.

An Expository Lexicon of the Terms, ancient and modern, in Medical and General Science, including a complete Medical and Medico-Legal Vocabulary. By R. G. Mayne, M. D. London: Churchill. 1853—1860. 8vo. pp. 1506.

The Medical Vocabulary; containing a concise Explanation of the Terms used in Medicine and its accessory Sciences. By Robert Fowler, M. D., Edinburgh, &c. London: Renshaw. 1860. Small 8vo., pp. 366.

Dr. Mayne's work, the result of many years' laborious research and judicious selection, has at length been completed by the appearance of the tenth part; and we are happy to say that the favourable anticipation we expressed at the time of the issue of the first fasciculus has been fully realized. The author has produced the fullest and most perfect medical lexicon in existence. His experience during the progress of the work, and the suggestions of literary friends, have enabled him to add a rather copious appendix, which, in a second edition, will, no doubt, be advantageously dispersed through the body of the work. With the present and concluding part, are given the preface to the volume, an explanation of the arrangement adopted, and an abridged dissertation on scientific nomenclature.

A couple of examples, taken quite at random, will best

convey an idea of the amount of information contained in this valuable work.

"Climacteric Disease.—*Pathol.* A term for that sudden change which occurs in many instances of advanced life, by which a patient falls off in flesh and strength before any loss of appetite or dyspeptic symptoms are complained of, and which appear to take place afterwards only by sympathy; it steals on so insensibly, that he is scarcely aware of his altered condition until his appetite fails, sleep leaves him, or what there is does not refresh him, his face becomes emaciated or bloated, his tongue white, his pulse more frequent; he has pains in his head and chest, his legs swell; but there is no deficiency in the quantity of the urine, nor any sensible failure in the action of the abdominal viscera, except that the bowels are more sluggish than they used to be. That this is disease, and altogether distinct from a natural or constitutional decay, is proved by the fact that recovery often takes place. The change referred to generally occurs about the fourth, or in the interval between the fourth and fifth climacteric periods. See *Climacteric.* Fr. syn. *mal climactérique.* Germ. syn. *Uebel klimacterisch.*"

It will be seen that the above extract contains one or two faults in composition, which do not, however, interfere with the immediate apprehension of the author's meaning.

"Clāvus, *i*, m. (*Claudo*, to shut or close; from its resemblance to the head of a nail, which this word also signifies.)—*Pathol.* A corn, or horny round cutaneous formation, the effect of continued pressure from tight shoes, generally on the prominent parts of the toes; it has a hard, dry, central portion, which, on every renewal of pressure, acts on the acutely sensitive substance beneath, causing an intensity of pain which only those afflicted can appreciate; also, a very acute pain confined to a small part of the head, as if a nail were being driven into it; usually connected with *Hysteria*, and then called *Clavus Hystericus*: also applied to a *condyloma* of the uterus, to a tubercle on the white of the eye, and to a certain degree of prolapse of the iris through an opening in the cornea, it being pressed flat, like the head of a nail. *Fr. anal. clavus,* m.; *clou,* m.; *cor,* m. Germ. syn, *Hühnenauge,* f.; *Nagel,* m."

As a dictionary of unusual technical terms, we have, during the years Dr. Mayne's lexicon has been in progress, most fully tested it; and we have only to repeat the opinion we have already expressed, of its adequacy as a most valuable companion to all engaged in medical literature, whether as readers or as writers.

The Dissertation published with the last part, comprising critical remarks on scientific nomenclature, and which now becomes the introduction to the work, consists of two principal

divisions, the first relating to terms common to Medical and General Science, the second to those peculiar to Medical Science. The six sections of the first division treat, respectively, of,—1. Terms, from the Greek, ending in *ĭdēs* and *ide*; 2. of Terms formed from Greek derivatives, ending in *ĭdeus*; 3. of terms from the Greek, ending in *adæ*, *idæ*, and *ĭdes*; 4. of terms from the Greek, in *ōdēs*; 5. of Botanical terms, ending in *ia*, forming the titles of classes and orders in the Linnæan and Jussieuan systems; 6. of the Greek Aspirate, and the misplacement of its symbol H, in Latin compound Terms and their English Analogues. The six sections of the second division are, in like manner, devoted to—1. The pathological term *Anasarca*; 2. the anatomical term *Gastrocnemius*; 3. the anatomical term *Psalloides*; 4. The pathological terms *Emprosthotonos*, *Opisthotonos*, *Pleurothotonos*; 5. the anatomical term *Platysma myoides*; 6. the terms—1, *Acidum*, *Causticum*, etc; 2, *Ephemera*; 3, Narcotics, Antispasmodics, Tonics, etc; 4, Fibrine, Stearine, Salicine, &c.; 5, the diphthongs æ and œ, and their English translation; 6, the terminal *ĭcus*; 7, the terminal *ismus*;, 8, the terminal *igo*; 9, the term *Lambdoidalis*, &c.; 10, *Zygoma*.

For the author's valuable remarks on the foregoing subjects, we must refer to his work; we would merely observe that we agree with him in his opinion that the final *e* in such words as *fibrine*, *caseine*, &c., ought to be rejected. We also think with him that in "spherical," "demon," "equal," &c., we have abundant precedent for changing the diphthong in hæmorrhage and allied words into *e*—a mode of spelling we have always adopted in this Journal. We are of opinion, however, that it might be better to let œ stand in *fœtus*, *fœtid*, &c., than to write *fetus*, *fetid*, although the latter course might seem to be authorized by the analogy of *economy*, *penal*, &c. The author acknowledges that a difficulty in the way of this change would present itself, in such words as *gonorrhœa*, *diarrhœa*, &c.; but this difficulty he proposes to meet by adopting these latter terms as they stand, without translation, and underlining or italicising them in writing or printing, as the case might be. We think, however, that such a course would give unnecessary prominence to the words in question.

In fine, we have only to recommend most cordially Dr. Mayne's valuable contribution to our standard works of reference, and to congratulate him on the successful and fruitful termination of his "twenty-eight years" of well-spent labour.

The publication of the other little work, whose title we

have given above, we consider untimely and uncalled for: appearing just as Dr. Mayne's encyclopedian labours were brought to a conclusion, gives it the character of an attempt to rob him and his publisher of the reward justly their due for both work and enterprise, by offering to the profession a cheap substitute. We regret to see that it has been favourably noticed by some of our London weekly contemporaries: not for this reason alone, however, but because, after referring to several of its pages, we have come to the conclusion that in its compilation the author has shown a lamentable want of judgment, and an amount of ignorance not at all to be overlooked in the present advanced state of medical and general science. As an example of the former, we need only to give a single extract:

"Arsecockle (Sc.) Formerly an inflamed pimple on the hips; in Scotland, at the present day, a similar one on any part of the body."

Now, we shall merely ask our readers, is this the style of information they would seek for in the pages of a medical vocabulary: would they not rather look for it in a dictionary of slang?

In the preface, Dr. Fowler states:—

"He has also sought to expunge all such phrases and words—coined by the forefathers of the profession—as appear to have become obsolete and disused, and not to have been recognised by the medical literature of the present century."

Now, notwithstanding this declaration, on looking through the pages, we find the following obsolete—nay, some of them ridiculous—terms explained:—Hapanismus, Hang-nail, Doloriferous, Knifesman, Lambative, Ropalic, Tractoration, Perplication, &c. The author's definition of one of these we must give, as another example of the information to be expected in the work:—

"Knifesman. One addicted to operate in almost all cases."

In fine, to show we are not giving an unfair verdict in utterly condemning this volume, we shall select at random a few examples, in proof of the thoroughly unscientific and most generally incorrect definitions contained in it:—

"Cerebellum (L.) The little brain, or the hinder and lower part of the brain."

"Dolicho-cephalæ (Gr. *dolichos*, long; *kephalē*, head). Races of men whose cerebrum completely overlaps their cerebellum."

"Half-caste. The offspring of mixed races."

"HECTIC FEVER (Gr. *ektēkō*, I pine away.) A slow, long-continued, or remittent fever which usually accompanies the termination of organic disease."

"OLEO-RESIN. A native compound of a terebinthinate oil and a resin, the proper juice of coniferous and other plants."

"PORRIGO (L. scurf in the head). A generic name of certain cutaneous diseases, principally characterized by an eruption of straw-coloured pustules, concreting into cellular scabs."

"ROSEOLÆ. A sort of hybrid exanthem midway in character between measles and scarletina [*sic*], but affording no protection against either."

And so we might go on quoting nearly the whole book; but we think we have said enough to show the worse than worthlessness of Dr. Fowler's Medical Vocabulary.

Anatomy of the Arteries of the Human Body, descriptive and Surgical; with the Descriptive Anatomy of the Heart. By JOHN HATCH POWER, M. D., &c., and B. WILLS RICHARDSON, F. R. C. S. I., &c. Dublin: Fannin and Co. 1860. 12mo. pp. 374.

DUE allowance being made for the occasional variety or anomaly in the origin, course, or distribution of any of the arteries in the human body, it may be safely asserted that this system of vessels, as found in the men of the present day, differs in no respect from the arterial system of the men of former times; and therefore a descriptive account, provided it be accurate, which is applicable to this branch of human anatomy at one period, will require but to be stereotyped for the readers of later times;—it is only the same story over again. But, although the purely descriptive anatomy of the human body has become as it were so beaten a track, that it is in our days extremely rare to find in it anything novel, or which has not been previously observed, it will be admitted that the rapid growth and development of our modern surgical improvements is attributable, in no small degree, to the mode in which surgical has been, so to speak, engrafted upon anatomical instruction in our schools,—the principles of the former being impressed upon the mind of the student along with the dry and otherwise uninteresting details of the latter. In no department of anatomy is the practical application of this principle of so much importance as in that which relates to angiographia, including not only the enumeration and nomenclature of arterial branches almost innumerable, and ramusculi without end, but the relative and

the surgical bearings of the principal vessels of the several regions.

About the year 1839, the late Dr. Flood, a most able anatomist, and a highly popular and successful teacher of the science, published a small work for dissecting-room use, entitled, "The surgical Anatomy of the Arteries, and descriptive Anatomy of the Heart; together with the Physiology of the Circulation in Man and inferior Animals." This work enjoyed a high and well-deserved reputation. In time, however, the original edition became exhausted; and Dr. Power then undertook the preparation of a new work, which should embody the essential parts of Flood's treatise, and at the same time should contain some important alterations and additions; the latter, as we learn from the preface, had reference chiefly to the anatomy of the heart, of the arteria innominata, of the subclavian arteries, and the operations performed on these trunks—of the axilla and the axillary artery;—the operations on the brachial artery, and the cure of aneurism of this vessel by compression;—to the circulation of the blood in the liver and in the kidneys,—to the iliac arteries, and the operations upon these important vessels and their branches;—and to the femoral artery, &c. Dr. Power's work was published in the winter of 1849-50, but has been for some time out of print.

The book which is now before us appears to be something more than a new edition of that of which we have just spoken; in fact, many of the anatomical descriptions appear to have been altogether rewritten, and are given in a style which, while it lacks the conciseness which characterised the language of the former edition, is, in our opinion, better calculated to convey accurate notions of the subject-matter of which it treats,—inasmuch as the smooth and flowing kind of composition is more easily read than the close and succinct, while the facts are not the less easily remembered. In the course of describing the various operative procedures connected with the several arteries which have been submitted to deligation, the author has recorded some important facts in connexion with improvements in surgery which have taken place since the publication of the former edition of his work; for example, among the operations on the femoral artery, we find the account of Professor Porter's operation of tying this vessel for femoral aneurism, by making a *transverse* incision through the integuments, and so reaching the artery whilst enclosed within the funnel, and of course before it had given origin to the profunda. The operation has been performed several times in this city with success. Again, in the chapter on ligature of the aorta, we find the details of *five*

cases in which this formidable operation has been performed; four cases are already well known; the fifth occured in the year 1856, and was operated on by Mr. South, who thus details the case :—

"The man was 30 years of age, and a hard drinker; had had a strange uneasy sensation two months before his admission, and six weeks afterwards noticed a small, hard, pulsating swelling in his right groin, which grew rapidly, and when admitted was as big as a goose-egg. Soon suffered paroxysms of violent pain, and the leg became numb. Eleven days after, the aorta was tied without difficulty by a cut from the tip of the 10th rib to superior iliac spine. In the course of a few hours, first one, and subsequently the other limb became discoloured; he was in profuse perspiration, and exceedingly restless; died 42 hours afterwards. Examination showed false aneurism of right external iliac artery."

By no means the least valuable feature of the present work is the circumstance of the descriptions being accompanied by upwards of sixty illustrations from the pencil of Mr Richardson. We have carefully examined the woodcuts, and consider them to possess several advantages over anything of the kind which has hitherto appeared in handbooks of anatomy; we allude in particular to the fact that they are sufficiently small to enable a student to take in with ease at one *coup d'œil* the relative anatomy of an entire region of considerable extent, and yet there is none of them constructed on so minute a scale as to have necessitated the sacrifice of clearness in any part of the detail. Those who are practically conversant with the difficulties which attend the preparation of drawings of this kind can best appreciate the importance of the service which the author of the present work has received from the valuable and truly artistic co-operation of that able surgeon, Mr. Richardson. It might here be suggested, *en passant*, that the usefulness as well as the beauty of the illustrations would be much enhanced by having the vessels coloured; any one possessed of a clear eye and a steady hand could easily colour them all in a short time,—the only materials required for the purpose being a little scarlet lake rubbed down with soft water for the arteries, and the same with the addition of a slight tint of prussian blue for the veins. We have just seen a copy coloured in this way, and can speak favourably of it.

The printing and the general finish of the work are highly creditable to the publishers, the type being clear and sharp, and the paper of a smooth and fair quality. We have with regret to notice some editorial errors, which, although not very serious, must still be considered as blemishes in an otherwise

irreproachable performance; we allude to such errors in phraseology as the "Operation *for* Lithotomy,"—a phrase which is placed at the opening of a chapter, as well as at the head of four consecutive pages, and may thus tend to lead students into error; next, to the misspelling of proper names,—that of M. Desault, as often as it occurs, having been spelled with double S; and, lastly, the violence which has been done to several words by the unskilful manner in which the printer has been allowed to deal with them in many instances, where the mechanical exigencies of the case requires a word to be cut into two parts, for example at the end of a line, thus:—des-cribe, hemorr-hage, Eus-tachian, anas-tomose, corres-pond, &c.

The greater number of the illustrations, the author informs us in the preface, have been executed by Mr. Oldham, of this city; and the remainder by Messrs. Butterworth and Heath, of London.

The Composition of the Urine, in Health and Disease, and under the Action of Remedies. By Edmund A. Parkes, M. D., Fellow of the Royal College of Physicians; Professor of Hygiene in the Army Medical School, &c., &c. London: Churchill. 1860. 8vo. pp. 404.

This excellent book fills a *lacuna* in medical literature, and supplies to the student and experimenter an accurate view of the present state of our knowledge of the important subject of which it treats. It is the only work, with which we are acquainted, that enables the student to ascertain the amount of labour bestowed already on the investigation of the urine, in health and disease, and to refer to the sources, where he can read for himself the results of the researches of the many explorers in this field of medical science.

It is divided into two books: The Urine in Health, and The Urine in Disease. The plan of both books is similar,—the one containing an introduction describing the normal constituents of the urine, and the other an introduction describing its abnormal constituents and sediments. In addition to the introduction, the first book consists of two chapters,—one on the variations of the urine during health from physiological conditions, and the other on its variations from the use of medicinal agents. In like manner, the second book contains, besides its introduction, three chapters, on the urine in acute and chronic disease (not renal), and on the urine in renal disease.

These chapters differ much in value and importance, some, as the chapter on chronic (non-renal) disease, being of the highest interest; while others, as that on the variations of the urine in health, from the use of medicinal agents, are admitted to be unsatisfactory by the author himself, who says:—

"It must be admitted that at present the experiments on no single medicine have been made sufficiently often; and there are few statements in the following pages [chap. ii.] which can be considered as proved, in the true scientific sense of the word."

Professor Parkes is well known as an original thinker and observer on the subject on which he writes, and consequently all his statements are worthy of more attention than those of any mere compiler of other men's labours would be. For this reason, and also because we believe that this book will be much read and studied, particularly by candidates for Assistant-surgeoncies, who form a highly-educated class of our medical students, we shall enter somewhat into detail in our review of some of the statements made by the author, who, we are sure, will take in equally good part our criticisms, whether we agree with him or differ from him—

"Amicus Plato, sed magis amica Veritas."

The most important portion of the whole book is, undoubtedly, the first section of the introduction to Book I., which contains a summary of the mean quantities of the normal constituents of healthy urine.

The constituents considered as normal are—

1. Water.
2. Urea.
3. Creatine, and creatinine.
4. Urine pigment.
5. Extractives.
6. Mucus.
7. Uric acid.
8. Hippuric acid.
9. Oxalic acid.
10. Formic acid.
11. Silicic acid, &c., &c.
12. Sulphuric acid.
13. Phosphoric acid.
14. Chlorine.
15. Lactic acid.
16. Ammonia.
17. Potash, soda, lime, magnesia.
18. Iron.

Some of these constituents are of much greater importance than others, and we shall confine our comments to these exclusively.

One or two preliminary observations are necessary on the method followed by the author to obtain his means.

1. He rejects (properly, as we think) all observations not referred to 24 hours as the unit of time.
2. He adopts, wherever it is practicable, the reference of the quantity of the *excretum* to the body-weight.
3. He limits his estimate to men between 20 and 40 years of age.
4. He rejects all determinations of quantity made on less than an average of three days; (and yet),
5. Admits experiments made by every variety of chemical manipulation; and
6. Made upon men of different countries, French, German, American, and English.

In the last two rules, we believe, the author has admitted sources of error and variations greater than those he has excluded by rule 4. The body of man in health is a well-ordered machine; and if it be made from day to day to do the same work, it will require the same amount of nutrition, and give rise to the same amount of waste; for this reason, we esteem of little value the painful experiments made by some observers, by taking an unusually large or small amount of food or exercise, and then determining the amount of excretions, such as urea, that have done their work in the system; for it is evident that the problem of the work done in the system by food or exercise is dynamical, and not statical, and that the body, under the same given conditions of food and work, acquires a stable condition of dynamical equilibrium, like the fly-wheel of a large steam-engine, which enables it to mask the effects of external forces applied to it, by virtue of the accumulated work which it has stored up. For this, and other reasons, we think that a single day's observation made upon a healthy man, who is living regularly, both as to food and work, is quite sufficient to determine his normal urinal constants. This, we believe, was the method followed by Mr. Haughton, in conducting the experiments published in this Journal, in August, 1859, and August, 1860; and, provided the condition of dynamical equilibrium be satisfactorily attained, we believe it to be quite sufficient for scientific purposes, and as satisfactory as eight or ten days' observation, with less attention to regularity of food and work would be.

We shall, therefore, compare the average results obtained

by Dr. Parkes, from men of different countries, and by different chemical methods, with those obtained by Mr. Haughton in his published papers; and the result of the comparison, we believe, will show that Mr. Haughton's method gives results, which do not differ materially (in most cases) from those given by Dr. Parkes; thus proving, as we think, that both methods are capable of giving the true means, while the advantage of simplicity, and fewer observations, belong to Mr. Haughton's method.

There is one very important omission in Dr. Parkes' results, viz., all mention of the particulars of food and work of the several subjects; this omission deteriorates from the value of his means, as it is known that, with variations of food and work, there are corresponding variations of urinary and other secretions.

The first excretion noticed is that of *water*, which is more readily measured than any other, and therefore there exists with respect to it less difference of opinion. Dr. Parkes gives (p. 5) the results of twenty-six observers, ranging from 35 ounces per day, to 81 ounces, the mean being **52·44** ounces daily.

Mr. Haughton found, by his method, a range from 34 ounces to 81 ounces, and a mean of **52·62** ounces daily,—the average of flesh-eaters being 47·3 ounces, and of vegetarians, 59 ounces daily.

This agreement of different methods is satisfactory, as it shows the true mean has been reached.

The second excretion, and the most important of all, is *urea*. Dr. Parkes' Table (including different chemical methods, and persons of different habits, and taking different foods) ranges from 286·1 grains to 688·4 grains per day; the mean being **512·4** grains, which is much nearer the higher than the lower limit, for reasons which might be easily assigned.

Mr. Haughton found a range from 315·00 grains to 677·25 grains, with a mean of **493·19** grains, which is almost exactly a mean between the extremes. This circumstance alone, we believe, entitles Mr Haughton's method of inquiry to be considered of more value than the mixed method followed by the twenty-four observers quoted by Dr. Parkes.

Either method appears to give the correct mean; and, as the methods differ, they may be regarded as mutually confirming each other's results, when they do agree. If, therefore, we group together men of every weight, food, occupation, and country, we may set down **500** grains of urea per day, as the natural and healthy discharge in the urine.

It is a matter of the highest moment to refer the discharge of Urea to the Body-weight; this is done for nine cases by Dr. Parkes (at p. 23), where he obtains the result, that each pound avoirdupois excretes daily **3·36** grains of urea.

The average weight of the eleven persons experimented on by Mr. Haughton, was 148.1 lbs., which gives a result of **3·33** grains of urea daily per pound avoirdupois of body-weight. The agreement of this result with that of Dr. Parkes is very close.

The third excretion we shall consider includes pigment, extractives, creatine, and creatinine; these are estimated separately by Dr. Parkes, and, taken together, they give a result of **1·142** grains per pound per day (p. 24). Mr. Haughton has furnished us with the following table of the pigment, extractives, creatine, and creatinine, passed by his eleven cases, which were all brought beforehand into the condition of stable dynamical equilibrium of waste and supply:—

TABLE I.—*Extractives, &c., passed per day by Vegetarians.*

No.	Body-weight in pounds.	Extractives, &c. in grains.	Grains per pound.
1	173	260	1·50
2	132	236	1·79
3	146	135	0·92
4	146	99	0·68
5	146	118	0·81
		Mean, . . .	**1·140**

TABLE II.—*Extractives, &c., passed by Flesh-eaters.*

No.	Body-weight in pounds.	Extractives, &c. in grains.	Grains per pound.
1	126	113	0·90
2	126	132	1·04
3	126	119	0·94
4	174	96	0 55
5	189	290	1·53
6	145	269	1·85
		Mean, . . .	**1·135**

The agreement of these means with that published by Dr. Parkes leaves nothing to be desired.

With regard to *Uric* and *Hippuric* acids, there is a striking difference between the results given by Dr. Parkes and those obtained in this city by Mr. Haughton.

Dr. Parkes gives **8·589** grains per day of uric acid as the quantity obtained by fourteen observers, and mentions quantities of hippuric acid ranging from this amount to 34·5 grains per day! as found by Lehmann and Weismann.

These results are certainly at variance with our experience of the urine of Irishmen, who pass, on an average, only **3·15** grains of uric acid, and no hippuric acid at all, except in rare cases. Can it be possible that the beer-drinking of the Germans tends to produce these abnormal compounds, particularly hippuric acid? If this be so, Charles the Fifth was not far astray, when he said he would speak German to his horse; for there appears to be a closer connexion between the frames of Germans and horses than has been hitherto suspected.

According to our judgment, both these acids are chance compounds, occurring in the urine of man as accidental portions of nitrogen that have escaped their natural conversion into urea, and as such must be regarded as essentially unhealthy products.

The mean amount of *sulphuric acid*, found by nineteen observers, is stated by Dr. Parkes to be **31·11** grains per day. Mr. Haughton's observations on this subject are limited to four vegetarians, from whose daily urine he found the following results:

TABLE III.—*Sulphuric acid passed per day by Vegetarians.*

No.	Weight in pounds.	Sulphuric acid in grains.	Grains per pound.
1	132	40·65	0·308
2	146	18·88	0·129
3	146	21·00	0·144
4	146	23·50	0·161
Mean .		**26·01**	**0·185**

The only inference we feel disposed to draw from the comparison of this table with that of Dr. Parkes is, that either the

Dublin bread contains less alum than German bread, or that we eat less bread than they do.

A similar observation may be made with respect to the quantity of *chlorine* passed per day, which depends rather on the habit of eating more or less salt with the daily meals—a habit which differs in every individual.

Dr. Parkes states that, as the result of sixteen observers, **126·76** grains of chlorine are passed per day. Mr. Haughton's observations on seven cases are irreconcilable with this statement, if we suppose the discharge of chlorine to have any physiological significance. They are as follows:—

TABLE IV.—*Chlorine passed per day by Healthy Men on a Mixed Diet.*

No.	Chlorine in grains.
1	26
2	49
3	13
4	116
5	40
6	36
7	79
Mean . .	**51·3**

With respect to *phosphoric acid*, which is of undoubted physiological importance in the urine, a serious difference exists between Dr. Parkes' table, p. 15, and the results published by Mr. Haughton in this Journal.

The total discharge of phosphoric acid per day, according to Dr. Parkes, amounts to **48·8** grains, obtained by twenty-five observers, the majority of whom, however, obtained their results by the use of Breed's method (phosphate of iron). Our own experience of this method is not such as to induce us to place much reliance upon it, as the precipitate appears to be of variable composition, according to varying conditions of precipitation; and therefore the phosphoric acid calculated by this method must be regarded as doubtful.

Mr. Haughton has informed us that all his results with

regard to phosphoric acid were obtained by actual weighing of the pyrophosphate of magnesia, and that he does not believe there is any reason to suspect any defect of phosphoric acid in them. These results are—for flesh-eaters, 37·07 grains per day, and for vegetarians, 26·7 grains; the mean of all being **32·36** grains per day. This result falls very short of that given by Dr. Parkes; and the difference cannot be explained, like that of sulphuric acid and chlorine, by accidental differences of diet; for the phosphoric acid unquestionably performs a more important office in the working of the animal frame than either of the others, and leaves the body in a condition different from that in which it enters it.

Notwithstanding the discrepancy between Dr. Parkes' and Mr. Haughton's results as to the total amount of phosphoric acid, there is an excellent agreement between the table (p. 17), which gives the relative proportions of phosphoric acid in combination with alkalies, and in combination with earths, and the corresponding relation deducible from Mr. Haughton's experiments:—

Ratio of phosphoric acid in combination with alkalies, to phosphoric acid in combination with earths, according to the results of seven observations published by Dr. Parkes, . . .	4·29
Ratio, &c., &c., according to Mr. Haughton's experiments, ...	4·15

This fact serves to show that the difference in the total amount of phosphoric is real, and possibly attributable to the greater amount of phosphoric acid taken in food by Dr. Parkes' observers, the greater number of whom are Germans.

The last point on which we have to compare Dr. Parkes' tables with the facts published in this Journal by Mr. Haughton, is as to the total amount of *solids* passed per day.

Dr. Parkes gives **945** grains as the mean of nine observers; while Mr. Haughton's mean is **957·81** grains per day. This agreement is what we should have expected from the previous agreement of their estimates of water and urea.

In the following table we sum up the results of Dr. Parkes (p. 24) and Mr. Haughton:—

TABLE V.—*Average discharge of Normal Constituents of healthy Urine per day, and per pound of body-weight.*

Constituent.	Ounces, or Grains, per day.		Drachms, or Grains, per pound.	
	Parkes.	Haughton.	Parkes.	Haughton.
1. Water . . .	52·44 oz.	52·62 oz.	2·90 drs.	2·84 drs.
2. Urea	512·40 grs.	493·19 grs.	3·36 grs.	3·33 grs.
3. Extractives, &c.	165·59 „	169·70 „	1·142 „	1·138 „
4. Uric acid. . .	8·591 „	3·15 „	0·059 „	0·021 „
5. Sulphuric acid .	31·11 „	26·01 „	0·214 „	0·182 „
6. Chlorine . . .	126·76 „	51·28 „	0·875	0·346 „
7. Phosphoric acid	48·80 „	32·36 „	0·336	0·218 „
8. Unaccounted for	51·75 „	182·12 „		
Total solids . .	**945·00**	**957·81**		

On examining this table, a difficulty suggests itself with reference to the solids unaccounted for. These must be the bases in combination with the various acids of the urine, and are soda, potash, ammonia, lime, and magnesia. Dr. Parkes' table leaves 51·75 grains of these bases, while Mr. Haughton has to explain 182·12 grains. Neither of these quantities, we believe, can be explained, as we shall now proceed to show. The principal base is soda, the atomic weight of which is not very different from the mean of those of all the bases. We shall therefore assume soda as the type base in the following calculation.

The acids given by Dr. Parkes require the following quantities of base to saturate them:—

Acids.				*Bases.*		
31	grains	SO_3	require	24 grains		NaO
127	„	Cl	„	83	„	Na
49	„	PO_5	„	42	„	NaO
207	grains			149 grains		

It appears, therefore, that the acids would require 149 grains of base; whereas there are only 52 grains of solids unaccounted for, and available for this purpose. This seems to us as if Dr. Parkes had estimated too highly the amount of acids in the urine.

On the other hand, Mr. Haughton's acids are not sufficient to account for the balance left unexplained in his table:—

Acids.				*Bases.*		
26 grains		SO_3	require	20 grains		NaO
51	,,	Cl	,,	34	,,	Na
33	,,	PO_5	,,	38	,,	NaO
110 grains				92 grains		

This calculation gives only 92 grains to explain the presence of 182 grains of solids not accounted for.

From the preceding reasoning, it would appear that Dr. Parkes has estimated the sulphuric acid, chlorine, and phosphoric acid discharged daily at too high a figure; and, on the contrary, that Mr. Haughton has estimated them at too low a rate. Unfortunately, we cannot tell in which of the constituents the error of either writer consists. If we were to hazard an opinion, it would be that Mr. Haughton has not given enough chlorine, and that Dr. Parkes has overestimated the chlorine and phosphoric acid.

The following considerations, based on Mr. Haughton's determination of the fixed salts in the daily urine of ten of his subjects, serve to throw some light on this interesting question:

TABLE VI.—*Alkaline and Earthy Fixed Salts passed per day.*

No.	Alkaline salts, in grains.	Earthy salts, in grains.	Total in grains.	Body-weight, in lbs.
1	214·54	23·46	238·00	126
2	221·94	22·68	244·62	126
3	222·04	11·96	234·10	126
4	364·00	20·30	384·30	173
5	145·00	15·00	160·00	174
6			405·00	189
7			297·25	145
8			421·20	132
9			261·00	146
10			249·20	146
Mean	**233·50**	**18·68**	**289·46**	

From this table it appears that the salts of soda, potash, lime, and magnesia, passed by the kidneys per day, amount to

289·46 grains. Adding together the acids and bases of Dr. Parkes' and Mr. Haughton's results, we find—

Fixed salts (according to Dr. Parkes) . .	356 grains.
„ (according to Mr. Haughton) .	202 „

This consideration shows an excess of 67 grains in Dr. Parkes' result, and a deficiency of 87 grains in Mr. Haughton's figures.

We believe that further experiments will be necessary to decide where the error lies; but that there is an error somewhere in the estimates of both writers, is, we think, certain.

Mr. Haughton's amounts of phosphoric and sulphuric acids were invariably determined by weighing, whereas his amount of chlorine was found by the volumetric process with nitrate of mercury. This process he never found to be satisfactory, as no two observers could agree when the precipitate ceased to be redissolved. For this reason, he thinks that the deficiency of acids rests with the chlorine, which ought to be about 100 grs. instead of 50 per day, in order to account for the balance of the solids.

From the preceding comparison of Mr. Haughton's determination of the constants of healthy urine, published in this Journal, with the results published by Dr. Parkes, it is evident that the most satisfactory agreement exists between these writers as to the mean amount of water, urea, extractives, and solids passed per day, by average men in health, with every variety of food and work; from this point, however, the two writers diverge,—Mr. Haughton laying more stress on the *occupation*, bodily and mental, of the subject considered, and Dr. Parkes attributing great importance to the *food*. If, however, we reflect that the occupation must regulate the food, the difference between them is less than would appear at first sight.

Dr. Parkes, in section xxvii. of Chapter I., renders ample justice to Mr. Haughton's speculations as to urea considered as the measure of work done. As these speculations are familiar to our readers, we shall pass over Dr. Parkes' account of them, observing only, that he does not agree with Mr. Haughton, in the opinion that mental work causes an increased elimination of urea.

Let us turn to the practical application of the facts laid down in the first part of Dr. Parkes' book. In pages 231–2, he lays down five rules for the determination of the physiological discharge of urea by a patient, under given circumstances—the amount of this discharge must be always calculated, before we

can determine whether he is passing too much or too little of urea—or of any other constituent.

These rules are—

1. Find weight in pounds.
2. Multiply the weight by given coefficients, to find mean healthy discharge of constituents.
3. Correct result for youth, age, and sex.
4. Correct for movement ($\frac{1}{10}$th to $\frac{1}{20}$th).
5. Correct for diet ($\frac{1}{6}$th to $\frac{1}{10}$th), ($\frac{1}{3}$rd, if fasting).

Dr. Parkes gives the following case in illustration (in quoting the case, we have corrected a typographical error):—

"I subjoin a case of mine, to show the working of the plan. A healthy man, aged thirty, on moderate hospital diet, weighed 114 lbs. Required the physiological amount of urea in twenty-four hours—

$$3{\cdot}53 \times 114 = 402{\cdot}42 \text{ grains.} \quad (1)$$

"Correct for moderate diet; deduct $\frac{1}{8}$th,

$$402{\cdot}42 - 50{\cdot}30 = 352{\cdot}12 \text{ grains.} \quad (2)$$

"Correct for bodily inactivity; deduct $\frac{1}{20}$th,

$$352{\cdot}12 - 17{\cdot}60 = 334{\cdot}52 \text{ grains per day.} \quad (3)$$

"The physiological amount determined by actual experiment (mean of six days) in this man was 346·7 grains, being only 12 grains above the calculated amount."

This calculation is, no doubt, satisfactory; but it may be made from a totally different point of view, as follows:—

From Mr. Haughton's Table I., it can easily be inferred that the *opus mechanicum*, in the average, may be considered to be represented by **66·74** grains of urea per day. Dr. Parkes deducts for total rest $\frac{1}{10}$, and for quietude the half of this, or $\frac{1}{20}$th of the whole urea—this is equivalent, on Mr. Haughton's theory to deducting 33·37 grains; and as the co-efficient for urea is 3·33, we find—

Healthy average urea per day = 3·33 × 114 lbs. = 380 grains.
Deduct for quietude, ... 33·37 „

346.63 grs. per day

This result is absolutely identical with that found by actual trial.

We believe, however, that the following rules, based on Mr. Haughton's theory, will be found the simplest and most accurate in practice, for determining the physiological amount of urea in a given case:—

1. Take the body-weight in pounds.
2. Multiply by 2, to find the *opus vitale* in grains of urea.
3. Add 33 grains for quietude, when there is no mental work.
4. If there be mental work, or anxiety, imagine an unknown quantity added.

To illustrate this view, we give the following cases, both taken from hospital patients, bordering on the uræmic condition:—

Case I.—G. B., Adelaide Hospital, aged 48 *circiter*, passed 31 ounces of urine in twenty-four hours, free from albumen and sugar, sp. gr. 1015·5; had had albuminuria some months before; symptoms of uræmia impending. The total quantity of urea in the 31 ounces of urine was 305 grains; and body-weight, 137 lbs.

Applying the preceding rules, we find—

Opus vitale = 137 × 2 = 274
Add for quietude . . 33

307 grains of urea.

Case II.—J. M'C., Sir Patrick Dun's Hospital, aged 50, *circiter;* body-weight, 134 lbs.; symptoms like last; never had albuminuria; trace of sugar in urine; passed 7½ pints of sp. gr. 1002·9; containing 261·4 grains of urea, lying in bed.

Opus vitale = 134 × 2 = 268
Add for total rest . . 000

268 grains of urea per day.

These two cases must be considered as bordering on the minimum discharge of urea, consistent with existence; they were both on a diet better than the average of hospital diet; and we do not see how they could be dealt with by Dr. Parkes' rule; neither case was in the uræmic condition, but both decidedly bordered upon it.

Dr. Parkes' rules would give the following results in these two cases:—

Case I.—Healthy urea . . . 137 × 3·53 = 483·6 grs. of urea.
Deduct for diet ($\frac{1}{8}$th) . . . 60·4 grains.
Deduct for quietude ($\frac{1}{20}$th) . . 24·1 „
Deduct for age ($\frac{1}{10}$th) . . . 48·3 „
—— 132·8

350·8 grs. per day.

This is 46 grains above the truth.

CASE II.—Healthy urea . . 134 × 3·53 = 473·0 grains.
Deduct for diet ($\frac{1}{8}$th) . . . . 59·1 grains.
„ total rest ($\frac{1}{10}$th) . . . 47·3 „
„ age ($\frac{1}{10}$th) . . . . . 47·3 „
—— 153·7

319·3 grs. per day.

This healthy physiological result is 58 grains above that actually found.

We believe the excess, resulting from Dr. Parkes' calculation applied to both these cases, is real, and represents the average *mental work* per day, which is already taken into account in his co-efficient **3·53**; but which was altogether absent in the patients under consideration.

From the length at which we have examined Dr. Parkes' results as to the normal constituents of the urine of health, we have but little space left for the consideration of the constituents of the urine of disease. We would especially direct the attention of the student to his sections (p. 244) on typhoid fever, (p. 270) on pneumonia (acute lobar), and pleurisy (acute).

The discharge of urea in pneumonia is greatly above the average, having been found, as a mean of twelve cases, to be **854** grains per day; while in pleurisy it only amounted to **348** grains, in a carefully examined case of a young man, the solids in the same case being only **482** grains.

The discussion of the phenomena of diabetes mellitus (p. 337) is the best we have seen.

The principal deductions to be made from this discussion are as follows:—

1. *Urea.*—There is an excretion of **811** grains daily (mean of twelve cases).
2. *Uric and Hippuric Acids.*—Not much altered.
3. *Sulphuric Acid.*—Greatly increased in amount.
4. *Phosphoric Acid.*—If altered at all, lessened in amount.
5. *Chlorine.*—Excretion of **247** grains daily (mean of seven cases).

These results we consider to be on the whole correct, with the exception of the asserted diminution of phosphoric acid. We have found this constituent increased largely, and in proportion to the other constituents of healthy urine. We shall take some other opportunity of stating our views on this subject, as well as on the proportion of sugar to urea, which is well discussed in pages 351–2.

Dr. Parkes admits three varieties of diabetes insipidus.

1. *Polydipsia.*
2. *Diuresis,* with deficient action of the skin and lungs.
3. *Diuresis,* occurring during the removal of dropsical affections.

On the first of these, we shall make a few observations. With regard to the assertion, that the urine exceeds the water of the liquid and solid food, we believe it rests on imperfect observation and calculation of the fluid in the solid food. According to Dr. Parkes, there are three varieties of polydipsia. They are the following:—

(α). *Polydipsia* with hydruria, and anazoturia, or deficiency of urea, and sometimes of phosphoric acid.

(β). *Polydipsia* (of Becquerel), with simple hydruria.

(γ). *Polydipsia,* with hydruria, azoturia, or increase of urea, and generally of chlorine—sometimes of phosphoric and sulphuric acids.

Dr. Parkes considers the variety (β), in which the only constituent changed is the water, to be of very doubtful occurrence. We have met with one such case, well marked; but consider it to be very rare.

In the third variety (γ) of polydipsia, the most marked increase in the constituents (in addition to the water) is in the chlorine; this increase is certainly real in the cases recorded by Dr. Parkes, as its amount greatly exceeds his average of health (126·8 grains) which we believe to be, itself, above the true average.

Our own limited observations on this form of diabetes confirm the accuracy of Dr. Parkes' conclusions.

We close our remarks, as we began them, by an expression of our opinion, that this book of Dr. Parkes is well timed, well written, and that it will rapidly take a high place among the scientific works which treat of the important subject with which it deals.

A Clinical Treatise on Diseases of the Liver. By Dr. Fried. Theod. Frerichs, of the University of Berlin. In 2 vols. Vol. I. translated by Charles Murchison, M. D., for the New Sydenham Society. London. 1860. 8vo, pp. 402.

This excellent work, so creditable to the distinguished author, whose reputation is scarcely less in these countries than in Germany, is one of the best productions of the New Sydenham Society. We look forward to its completion with the utmost interest, and are glad to observe that the subscribers to that most

useful society may confidently expect the early issue of the second volume. A short, but very satisfactory, "historical introduction" is a fitting preliminary to the work. The most curious subject for reflection, in connexion with this branch of the inquiry, is the diversity of views concerning the importance of the liver in the animal economy at various periods of medical history. The notions of Galen, which seemed so extravagant to the anatomists of the seventeenth century, have now again been upheld by the most recent and authoritative researches:—

"Galen looked upon it as the focus of animal heat, and as the organ intended for the formation of blood, and for the origin of the veins. According to him, the metamorphosis of chyle into blood commenced in the portal veins, but was completed in the liver, which organ, during the process, separated, as waste matter, from the blood, the yellow and the black bile,—the former passing to the gall-bladder; the latter, to the spleen."

Much of this statement seems to be now irrefragably based on fact; but the very outline of the dogmatic pretensions of the physiologists here placed before the reader, should teach a lesson of modesty, and induce us to pause before yielding an unlimited assent to the views now dominant.

The second chapter, on "the relative size and weight of the liver in health and in disease," consists mainly of a series of tables, in which are arranged the results of the author's observations. We may in vain search elsewhere for so extensive and accurate a series of researches on these important subjects. They form a necessary groundwork for the elaborate clinical inquiry which immediately follows, and must be studied with care by those who would derive all the advantage the book is calculated to impart. The modifying influences of "sex," "age," "ingestion of food," and "degree of congestion of the liver," are successively alluded to. Here, too, the strange contradictions among the authorities strike the eye, even when involving questions of weighing and measuring. Bartholin estimated the relative weight of the liver in proportion to that of the body, as 1 to36. Haller, as 1 to 25. The average weight of the gland was considered by Haller to be 3·7 lbs. Cruveilhier's estimate was 3 lbs. "According to my experience," says the author, "the relative weight of the liver in healthy individuals may vary from one-seventeenth to one-fiftieth of that of the body; and in adults it fluctuates between one-twenty-fourth and one-fortieth; the absolute weight at

this period of life reaches from 1·8 to 4·6., English lbs. avoir-dupois."

These numbers certainly exhibit a wide range of variation within healthy limits; and the known care of the author gives a sufficient guarantee for the accuracy of the statements; but we believe that few clinical observers, in their efforts to determine the hypertrophy or atrophy of the liver, pay sufficient attention to these fundamental facts.

The ingestion of food exercises a powerful influence over the volume of the liver. Dr. Frerichs found that in a healthy individual, aged 27, who, owing to a fall from a scaffold, died with a full stomach, the relative weight of the liver was found to be as 1 to 26·5; in another person, aged 36, who perished under similar circumstances, it was as 1 to 37; on the other hand, in a man aged 25, who died of trismus, after three days' complete abstinence, it was 1 to 40; and in a woman, 33 years of age, who died from burning the pharynx with sulphuric acid, after seven days' fasting, it was 1 to 50. These figures offer indisputable proof of the vast influence of the digestive processes on the liver,—a doctrine which indeed is universally entertained, even among the vulgar. But, as is judiciously noted by the author, we also infer from them that a strict diet must play an important part in the management of chronic congestions of the liver. In one sense, this, too, is a pretty generally diffused dogma, though it may be doubted if the considerations connected therewith meet with the practical assent they deserve.

The third chapter contains a sketch of the relative sizes and forms of the liver in its morbid conditions, and of their diagnostic value; elaborate tables illustrate the subject; and the varied information obtained by palpation, percussion, and inspection, is fully set forth. Both here and in other parts of the book, great attention is bestowed on the tight-lace liver: much more than is usually allowed to this artificial anomaly in English works on the liver. In reference to the *mobility* of tumours connected with this organ, it is observed, that "in the case of the lobes separated by tight-lacing, it is not unfrequently so great, that we can turn the tumour, and lay it upon the upper surface of the organ. In exploring the hepatic region of females, this observation merits to be borne in mind, so long as women try to de-naturalize the feautiful frame with which nature has gifted them.

Cruveilhier's remark, that a liver whose position has been changed by tight-lacing may be diagnosed from that which results from inflammatory enlargement, by the thinness of the

border in the former case, is stated by Frerichs to be an error, inasmuch as the margin of the tight-lace liver is always rounded and nodulated.

The section on the application of the results of observation to diagnosis is judicious, but presents no novelty. It is insisted upon that structural diseases of the hepatic parenchyma give rise to abnormal positions of the organ, only when they are accompanied by softening. This is the case in fatty degeneration, and in acute atrophy. The effects of these diseases vary according to the degree of softening. When this is moderate, as may happen in fatty liver, the axis falls downwards, and the gland protrudes farther below the margin of the ribs. Hence a fatty liver usually appears, upon percussion, to be larger than it really is. When the softening is considerable, as in acute atrophy (in which the gland is flexible, like a piece of cloth), the liver folds upon itself, and collapses towards the vertebral column, and the space corresponding to it in front is filled up by intestines containing gas. The hepatic dulness then disappears entirely in front, whilst posteriorly it may still be detected (p. 50). This last is a most weighty point as regards the diagnosis of acute atrophy, the most formidable and the most obscure of hepatic disorders. Scarcely any other deviation from the normal state is sufficiently characteristic to enable us in ordinary cases even to suspect the existence of that disease. The diagnosis of increased volume of the liver from empyema is, in some cases, surrounded with many difficulties; but the author draws attention to the valuable aid derived from a careful determination of the *mobility* of the liver on a deep inspiration. Here, of course, we are mainly guided by its lower margin. When the mobility remains unchanged during a full inspiration, we infer that the diaphragm, and, with it, the liver, are not depressed by pleuritic effusion, but that an increase in size of the organ has altered the situation of its upper and lower margin. The reverse side is not always true, as in some cases the diaphragm loses its contractibility through fatty or fibrous degeneration.

The fourth chapter consists of an extended discussion on jaundice, wherein the author's originality and masterly clinical knowledge are admirably displayed. We are unable to do justice either to the writer or to the subject, without giving an extended *resumé* of this part of the treatise.

It is justly stated *in limine* that, although copious materials have been accumulated, in the course of time, bearing on this subject, yet the most important fundamental questions have never received a satisfactory explanation. Passing over

minor detailed views, we may easily resolve past opinions into the two following theories:—

1. One of these theories is, that the seat of these diseases is to be looked for in a disturbance of the functions of the liver; that they arise from abnormalities in the secretion, or in the excretion of bile, and that thus they exhibit symptoms indicative of derangement of the liver.

2. The second theory is that, under morbid conditions of the system, substances are formed in the blood without the co-operation of the liver, which, in colour and other properties, resemble the ingredients of bile, if they are not identical with them, but which only assume a pathological importance from their quantity being in excess (p. 79).

The first of these theories is the time-honoured one of Hippocrates and Galen; and, in spite of numerous revolutions, has held its ground to near the present time. That obstruction to the excretion of bile is a cause of jaundice, may be considered a settled doctrine; but, in the absence in certain cases of material proof of such obstruction, how can the jaundice be accounted for? The explanation of such forms which has obtained general assent in this couutry is that which has received the sanction of Drs. Watson and Budd, namely, that the bile does not originate in the liver, *but is previously generated in the blood*, and that thus anything which interferes with its *elimination* may give rise to jaundice. It is remarkable that this last view is still maintained, in the face of the chemical analysis of portal blood, and of the experiments upon the extirpation of the liver.

The second theory has also had supporters from an early period of medical history. Senac held that the red portion of the blood was the peculiar material of the bile, and that it assumed a yellow colour when it became putrid, or otherwise decomposed. Lately, since the doctrine that the hematine of the blood forms the basis of all pigments has become more generally accepted, many observers have referred the jaundiced tint of the skin, present in pyemia, in putrid infection, and other allied conditions, without any morbid condition of the liver, to a direct metamorphosis of the hematine into a yellow substance, similar to, if not identical with, bile-pigment. This view has obtained fresh support from the experiments of Virchow, which show that, under certain circumstances, a yellow substance is formed from the hematine, which, in its relation to solvents and re-agents, bears a close resemblance to cholepyrrhin. The discovery of Zenker and Funke, that bilifulvine can easily be transformed into hematoidine (a derivative of he-

matine), also shows an intimate relation between the bile-pigment and the red matter of the blood. Further, we are told in the preface that, "since the publication of the German edition of the first volume, certain experiments have been performed in Frerich's laboratory by his assistaut, Dr. Valentin, which tend to show that one of the colouring-matters of bile consists of hematine, the substance which is known to be derived from blood-pigment. Valentin has succeeded in detecting crystals of hematine in gall-stones, in the bile of men and animals, and in the tissues and secretions of jaundiced patients. The addition of chloroform is found to dissolve the hematine with a yellow colour; and, from this solution, red and brownish-red, lancet-shaped, and rhomboidal-prismatic crystals separate, which correspond in every respect with those of hematine" (p. xviii.). These two theories of bilious affections have never received a universal application, and were employed to explain the origin of those forms of jaundice only, in which no proof existed of any obstruction to the escape of bile. Their main value consists in the necessity for some hypothesis for explaining observations where no anatomical obstruction can be detected. "In such cases can we ascribe the jaundice to an accumulation of bile in the blood, owing to something which interferes with its secretion; or are we to adopt the theory of a direct crumbling down of the blood-corpuscles, or red matter of the blood, into bile-pigment?" (p. 82). There are numerous well-established facts which seem to negative the opinion of Budd, that an imperfection in the secreting functions of the liver may be the cause of jaundice. All the efforts made to detect the essential elements of the bile in the blood generally, and in that of the portal vein in particular, have been nugatory. Frerichs relates a case (No. xxix.) where the secretion of bile was completely arrested in consequence of fatty degeneration of the liver, in which the gall-bladder was found empty, and the biliary ducts coated with a greyish mucus; and, notwithstanding, the skin was of a chalky paleness, and the urine not tinged with bile-pigment. But the most decided proofs of the formation of bile in the liver are afforded by the experiments of Kunde and Moleschott, which show that not a trace of the elements of bile could be detected in the blood, lymph, urine, or muscular tissue of frogs, several weeks after they had been deprived of their livers.

As regards the second hypothesis, the spontaneous conversion of the red matter of the blood into bile-pigment, no objection can be made on the score of possibility, considering the known intimate relations of hematine and cholepyrrhin. But

no one has yet succeeded in manufacturing bile-pigment from the red colouring matter of the blood. It remains to be proved that this metamorphosis really takes place in the living body. How, then, are those cases of jaundice to be explained which occur without any remarkable obstruction to the excretion of bile—cases which include, in addition to the jaundice of pyemia and of allied conditions, the jaundice of chloroform and other forms of intoxication, that of pneumonia, intermittent fevers, bilious fevers of marshy districts, inflammation of the portal vein? It is necessary here to go back, and search after the causes which can give rise to accumulations of bile in the blood. Excluding, now, polycholia, and assuming the formation of bile to be a constant quantity, these causes may be said to be twofold:—

1. Through increased absorption of bile from the liver into the blood.

2. Through some alteration in the metamorphosis of substances contained in the blood[a].

The mechanism of biliary obstructions is more complicated and obscure than is generally imagined. We must conclude that an increased absorption of bile into the blood is dependent upon a difference in tension of the contents of the hepatic cells and blood-vessels. Such a condition may arise either from obstruction of the bile-ducts, by which the pressure on the side of the cell-contents is increased, or from obstruction to the flow of blood in the portal system, and consequent diminution of the pressure on the side of this fluid. All mechanical obstructions, interfering with the flow of bile into the larger or smaller biliary ducts, produce jaundice in the first of these two ways. The fundamental principle is *increased pressure* on the side of the bile. Its outflow seems principally brought about by the *vis a tergo* of the secretion constantly pressing onwards. Muscular contraction, causing spasm of the ducts, has been long considered a cause of jaundice; but this, apart from the existence of a local cause, as a concretion, must be negatived. Paralysis of the ducts has been invoked to the same end. All *à priori* argument is against this supposition, and the direct experiments of Frerichs and Bernard contradict it. In reference to the circumstances influencing the flow of bile, we must not leave out the compression exercised upon the abdominal organs by the respiratory movements. The

[a] The reader must take care to make an important correction in the text, and refer to the errata. Since the publication of the first German edition, new researches have led the author to modify previous statements.

want of this influence may be insufficient to give rise to jaundice, but it may co-operate in the production of the result.

The second mode by which bile is made to enter the blood, by the obstruction to the circulation, and the consequent diminution of the pressure on the side of the capillaries of the portal vein, is estimated with difficulty. There can be no doubt, however, that the tension of the capillary vascular system in the liver is diminished, and the entrance of the biliary contents of the hepatic cells into the blood facilitated, by obstruction of the main trunk, or of the larger branches of the portal vein. Here we find an explanation of the jaundice of new-born children, a diminution of the tension of the hepatic vessels occurring, directly after birth, when the portal vein ceases to receive blood from the umbilical veins. The great hemorrhages of yellow fever must also produce variations in the pressure exerted on the side of the hepatic vessels. The importance of the distribution of the blood in the liver must not, therefore, be lost sight of in the production of jaundice.

We arrive now at the *second* cause of the accumulation of bile in the blood—that arising through some alteration in the metamorphosis of substances contained in the blood. The original researches of the author are prefaced by the following statement, which we extract from the page of errata:—

"The bile-pigment is so intimately related, on the one hand, to the red matter of the blood, and, on the other, to the colourless biliary acids, as to justify us in referring its origin to one or the other of these sources.

"The intimate relation subsisting between the bile-pigment and the colouring-matter of the blood is indicated by facts which have been already mentioned, but more particularly by observations which have been recently made in my laboratory by Dr. Valentin, according to whom, a portion of the colouring-matter of the bile dissolves in chloroform; and from this solution a crystalline substance may be obtained, presenting all the characteristics of hematoidine. From this it appears possible, nay, probable, that, as in extravasations, hematoidine may be developed from blood-pigment; so, in like manner, in the vascular system, and in the liver, the colouring-matter of bile may originate from the same source. Hitherto, however, no one has succeeded in obtaining bile-pigment directly from the red matter of the blood. The second view rests upon the following facts:—The pure colourless acids of the bile may be transformed into *bile-pigment*, with all the properties characterising this substance. Such a transformation takes place, not only under the influence of re-agents, but it also follows the absorption of the acid substance (into the blood of living animals), and is in a measure dependent upon this. By the action of concentrated sulphuric-acid

upon colourless bile, there are formed colour-producing substances (chromogene), which, upon exposure to the atmosphere, and still more rapidly on the addition of nitric acid, exhibit alternations of tints corresponding, in every respect, with bile-pigment. The same pigments and colour-producing substances (chromogene), which in their properties precisely resemble cholepyrrhin, are produced by the injection of large quantities of colourless bile into the vascular system of living animals. In this case the acids of the bile are transformed in the blood into pigment, under the influence of respiration."

These remarks are supported by numerous experiments, fully detailed in the appendix. Certain facts suggest the supposition, that, under various pathological conditions, the metamorphosis of bile in the blood is incomplete, and that thus a sufficient quantity of bile-pigment remains in the blood to give rise to all the symptoms of jaundice.

This theory of jaundice has been impugned by Dr. Kühne, since the appearance of Frerichs' work; an abstract of his paper may be found in Dr. Beale's Archives of Medicine, vol. i. This physician denies that the biliary acids are decomposed in the blood; and asserts that, in jaundice from closure of the ductus communis choledochus, the urine *always* contains biliary acid, as well as bile-pigment (see preface). Dr. Kühne's explanation of Frerichs' experiment, which showed a great quantity of colouring-matter in the urine after the injection of biliary acids into the blood, is, that the colouring-matter which is developed under these circumstances is due to the property possessed by the biliary acids of dissolving the blood-corpuscles, and of thus setting free a quantity of hematine, which is converted into bile-pigment.

The results attained by Frerichs have been confirmed by many subsequent observers, especially by Professor Staedeler, of Zurich, and Dr. Neukomm. This latter experimenter has availed himself of a test more delicate than either Pettenkofer's or Hoppe's (which was used by Kühne). We now quote from the lucid preface by the translator:—

"These several processes were applied to the examination of two specimens of jaundiced urine from the human subject, and to that of the urine of seven dogs, into whose blood solutions of the biliary acids had previously been injected. In both the specimens of jaundiced urine, Pettenkofer's test gave a negative result; but the author's process indicated the presence of minute traces of the biliary acids. It is shown, however, that the quantity of these acids was so small (less than one-thirteenth of a grain troy in forty-two fluid ounces) as to bear a very small proportion to the amount

which must have entered the blood. Of the seven dogs, into whose blood *large quantities* of the colourless solutions of the biliary acids were injected, in all the urine became coloured with bile-pigment within a certain number of hours after the operation; in none could any bitter taste be distinguished in the urine; in none did the urine exhibit the slightest reaction with Pettenkofer's test; and in two cases only did the author's test indicate the presence of traces of the biliary acids in this secretion."

Now, let it be observed that Kühne's experiments fail in proving that the colouring-matter of the urine originates from the colouring-matter of the blood, and not from the transformation of the biliary acids; and they likewise fail in accounting for the disappearance of the biliary acids injected into the blood, in any other manner than that suggested by Frerichs' (Translator). A high probability, therefore, exists in favour of the author's doctrines of the genesis of icterus, which he thus sums up:—

"We have become acquainted with three causes of icterus:—

"1. Obstruction to the escape of bile.

"2. Diminished circulation of blood in the liver, and consequent abnormal diffusion.

"Both of these conditions give rise to an increased imbibition of bile into the blood; and, in both cases, the liver is more or less deeply implicated.

"3. Obstructed metamorphosis, or a diminished consumption of bile in the blood.

"This cause is independent of the liver; and, so far as we as yet understand the matter, is chiefly influenced by the composition of the blood, and by everything which essentially limits or modifies the processes of metamorphosis within the vascular system."

The next 100 pages of the treatise are occupied with an elaborate clinical inquiry into the various forms of jaundice, the subject being illustrated by numerous cases. We had marked many parts for quotation, but must now hasten on to the remaining chapters.

The fifth chapter deals with suppression of the functions of the liver, acholia, and its consequences. Here, too, jaundice is one of the symptoms of the disease; but this form of disorder always comes on sporadically, depends upon a suspension of the secretion of bile, and, as a general rule, is produced by acute atrophy of the liver, and in some instances by other structural diseases, such as cirrhosis, fatty degeneration, &c. It is much more dangerous than the former varieties of jaundice, and almost always terminates fatally. In infectious diseases, attended with jaundice, enlargement, and albuminous infiltra-

tion of the liver and kidneys, are not unfrequently present. This constitutes the commencement, from which proceeds destruction of the hepatic cells. In this way, typhus and allied processes may become the remote causes of acholia.

The acute or yellow atrophy of the liver is a disease still but imperfectly known; and what accurate information we have concerning it, dates no farther back than the time of Morgagni. It must be admitted, too, as observed by the author, that the present materials at hand for enabling us to comprehend the precise characters of the malady, are of a very heterogeneous nature. The symptoms are sometimes preceded by a preliminary stage, but at other times manifest themselves directly. In severe cases, the scene terminates at the end of twelve or twenty-four hours; in other cases, after two or five days; it is scarcely ever prolonged for a week. When precursory symptoms exist, they present nothing characteristic. Sooner or later, a slight jaundiced tint of the skin supervenes on the other pyrexial and gastro-enteric phenomena. This jaundice may exist in the simple form from eight to fourteen days, or even longer, before the local changes in the liver and spleen, the hemorrhages, and the serious derangements of the nervous functions, which characterise the affection, become apparent. The fully-developed disorder is attended with cephalalgia, delirium, generally of a noisy character, convulsions, and coma. The pupils become larger, react slowly with light, and the respiration becomes sighing, intermittent, and stertorous. The pulse, *at first*, *slow*, at the outbreak of the nervous symptoms increases in frequency, and gradually rises to 110 or 120. It also presents remarkable variations in its frequency and volume. Towards the close, the pulse always increases in frequency, and becomes smaller. In most cases, the abdomen is tender on pressure; even during the coma, the application of the hand to the hypochondriac regions generally produces distortions of the features, and complaints. The colour of the skin increases in intensity; petechiæ and extensive ecchymoses occur, along with hemorrhages from the nose, the vagina, the stomach and bowels, and bronchi. The urine contains brown bile-pigment and deposits a precipitate, containing amorphous mucus, coloured epithelium of the kidneys and urinary passages, and also needle-shaped crystals (tyrosine) coloured yellow. Sometimes there is albumen. Death usually occurs amidst increasing coma, and other symptoms of cerebral paralysis.

Above all things, as regards the symptomatology of this disease, it is important to bear in mind that "the extent of the hepatic dulness diminishes more and more, as the disease ad-

vances; and not unfrequently the dull space disappears completely, without there being any tympanitic distention of the bowels to account for it." The first three illustrative cases occurred to women in the seventh month of pregnancy. We are unable to give their details fully, but give an abstract of the necropsy of the second:—

On the 17th of January, 1858, she was attacked with symptoms resembling those of acute catarrh of the stomach; intense jaundice supervened; and on the evening of the 23rd, she died. The urine was dark-coloured, giving the reaction of bile-pigment, *but not of the biliary acids.* After standing, a greenish-yellow sediment was deposited, consisting entirely of acicular crystals of tyrosine, aggregated together in globular masses. When a drop of urine was evaporated, the residuum was found to consist of crystals of leucine and tyrosine, partially saturated with colouring-matter. At the necropsy, the skin was of a yellow colour. The large intestine contained scybala of fecal matter, but faintly tinged with bile. The mucous membrane of the bowel was everywhere pale and anemic. Ecchymoses existed in the mesentery. The spleen was enlarged by about one-third, soft, and of a pale red colour.

The liver lay collapsed against the posterior wall of the abdominal cavity; anteriorly, it was completely covered by the folds of the small and large intestine; it was dry and soft; its capsule was puckered and opaque, and its margin sharp. Its weight, in relation to that of the entire body, was only 1 to 68·5. In healthy females of the same age and weight, the ratio is 1 to 28. It was also considerably diminished in size: the left lobe measured 3 inches transversely, and $5\frac{1}{4}$ from before backwards; and the right lobe, $5\frac{1}{4}$ inches by $5\frac{1}{2}$; the thickness was $1\frac{1}{3}$ inch. The gall-bladder contained a small quantity of grey mucus. The tissue of the liver felt flabby, in some places it was congested; the ramifications of the portal vein surrounding the lobules were distended, whilst the centre of the lobules presented a citron-yellow colour. Here and there were small ecchymoses. A greyish-yellow substance was observed in the spaces between the lobules. In the left lobe, where the morbid process had advanced farther, this inter-lobular substance was still more extensive; the cut surface here presented an ochre-yellow colour, interspersed through which were distinct ramifying streaks. The secreting cells were completely disintegrated, and in their place were found numerous drops of oil and brownish-yellow molecules; it was only in the rounded border of the right lobe that a few isolated cells loaded with oil could be detected. The cut surface became covered with

a greyish-yellow film, consisting of globules of leucine mingled with crystals of tyrosine. Urea and phosphoric acid were absent from the urine. Its abnormal constituents have been already referred to. The blood in the heart, and in the venæ cavæ, contained a small quantity of leucine; and larger quantities were found in the cerebral substance, the liver and the spleen: these last organs contained a much larger amount than corresponded to what was found in the blood. The liver contained a considerable amount of tyrosine, whilst in the spleen this substance could not be detected with positive certainty,

The 18th observation is a case supplied to the author by Rühle—abdominal typhus, profuse epistaxis, violent delirium, jaundice on the fifth day, disappearance of the hepatic dulness, general muscular tremors, coma, death on the 8th day, are the conditions mentioned at the head of the case. The necropsic phenomena were small, shrivelled liver, with partially disintegrated cells, and empty bile-ducts, tumefaction of the spleen, deposits in Peyer's patches, and in the solitary glands of the ilium. But, as Frerichs observes, it is a matter of question whether this can be considered a case of typhus, complicated with jaundice, or of acute atrophy. It appears, indeed, often impossible to draw a sharp definition between the two conditions just named. The diffuse infiltration of the hepatic parenchyma, and the impaired secreting functions of the organ, which occur not unfrequently in severe cases of typhus, are abnormal states which differ from acute atrophy, with complete arrest of the hepatic functions, more in degree than in nature. In both cases, there is an exudation, which deranges the nutrition and the functions of the gland.

The author, after giving an account of the symptoms derived from the skin, the organs of circulation, the respiration, the organs of digestion, the urinary organs, and the nervous system, discusses the duration and mode of termination of the disease. Out of the thirty-one cases tabulated, the date of the commencement of three was unknown; in the remaining twenty-eight cases, the fatal termination occurred during the first week, 13 times; during the second week, 6 times; during the third week, 5 times; during the fourth week, 4 times.

A few cases have been recorded, as by Griffin and Hanlon (whose observations are familiar to us in the classic pages of Graves), where a favourable issue occurred; Budd also makes mention of such a case. But it may be made a matter of question, how far the diagnosis in these cases was perfectly correct. We can, at any rate, fully concur with the author, that "when

the anatomical lesions, from which this disease is thought to originate, have made considerable progress, and the larger portion of the hepatic cells have become disintegrated, one can understand that a cure can no longer be thought of." To the anatomical lesions we need no further refer, as the necropsy above given supplies us with the requisite data.

The nature of the disease is involved in much obscurity. It has been regarded as a process of bilious liquefaction (Rokitansky); as involving mainly a considerable impairment in the nutrition of the hepatic cells, and their subsequent disintegration by fatty metamorphosis (Henoch); paralysis of the bile-ducts has been also invoked (Von Dusch). These views are easily controverted; but it is more difficult to supplant them with a well-based hypothesis. Bright was of opinion that the disease essentially consisted in diffuse inflammation of the liver; and this view has obtained many supporters in Germany, of late, who have explained the destruction of the cells by a fatty degeneration, arising from an acute exudation-process. The author, though appreciating the difficulties which surround this doctrine, inasmuch as the familiar process of fatty degeneration offers no example of such rapid destruction of secreting structures, yet believes that an exudation-process constitutes the starting point of the disease—

"The bile-ducts at their origin are compressed at an early period, by the exudation at the periphery of the lobules; as a consequence of this, the secretion formed in the central portion of the lobules stagnates, and passes into the central veins, and so into the general mass of the blood."

An objection to this theory is alluded to by the author, but very summarily dismissed. We, on the contrary, think it fatal to this doctrine of exudation. It is, that no hyperemic enlargement of the organ can be detected during the progress of the disease. That the cell-structures of the organ break down under a certain acute destructive process, more or less allied to fatty degeneration, must be allowed; but of the steps preliminary to this process we absolutely know nothing, but must await further inquiry.

We must predicate, also, this last remark of the etiology of the affection, about which there is no satisfactory information. It is interesting to note that of the 31 cases, 22 were females; and of these 22, one-half were attacked during pregnancy. As regards diagnosis, the characters of the urine, bloody vomiting, jaundice, the remarkable nervous phenomena, and above all the diminution in size of the liver, must be chiefly relied upon;

but in the early stage of the disorder, a diagnosis is almost impossible.

The treatment consists of strong purgation, and, when there are severe pains in the liver, the application of leeches, cupping-glasses, and cold cloths. In full-blooded individuals, Frerichs advises venesection. Allusion is made to a recommendation of Dr. Corrigan, the administration of emetics. The hemorrhages require especial attention.

The author goes on to discuss disorganisation of the liver, induced by—1, Obstruction to the flow of bile, from impermeability of the great ducts; 2, By cirrhosis; and, 3, By fatty degeneration, properly so-called; and devotes the sixth chapter to the description of chronic atrophy.

This form of atrophy makes its appearance in connexion with various morbid conditions; but the author excludes those forms produced by the development of new growths in the liver, as cancer and echinococci, or having relation to cirrhotic wasting, or cicatrization. The author here includes only those forms of atrophy which are unconnnected with any other obvious disorder of the organ, and may, therefore, lay claim to individual existence. In these forms the liver is usually of a brownish, or reddish-yellow tint, contracted in size; the lobular outlines very indistinct, or even annihilated; and the cells are small, angular in outline, having scanty or no granular contents, with intermixture of cells which have undergone fatty degeneration. The portal vein is usually considerably enlarged, as far as its subdivision into capillaries at the periphery of the lobules. The walls of the enlarged veins are sometimes normal, at other times they present a remarkable thickening of the sheath, formed by Glisson's capsule. The capillaries themselves are in a great measure destroyed; they become filled with brown molecules, or flakes, or granules of black pigment. The hepatic cells, too, often contain pigment granules.

These conditions of the liver are connected with very diverse states of the other abdominal organs, most of which, however, arise from chronic exudation-processes. The author's cases show ulcer of the stomach (No. 23); dysenteric cicatrices (No. 26); chronic dysentery (No. 27); fibrous thickening of the mesentery, with cicatrising ulcers in the small intestine (No. 28), to co-exist with, and apparently to originate, the morbid states of the liver. But other causes of great interest occasionally preside over the formation of this form of atrophy. Thus, in the twenty-fourth and twenty-fifth cases, the disordered nutrition of this organ supervened on intermit-

tents; and was, probably, directly caused by the blocking-up of the hepatic capillaries with pigment-granules. The twenty-ninth case displays the co-existence of atrophy with thrombus of the portal vein—itself secondary to a long-persistent thrombus of the pulmonary artery. The thirtieth case is also one of constriction of the portal vein, secondary to ulceration of the duodenum. The symptoms of this condition present very marked differences from those occurring in the acute disease, and lead us, inevitably, to the conclusion that the words acute and chronic give but a very imperfect idea of the total difference between these disorders. But this is a remark which has a far more general application than as refers to the matter in hand. Time offers, probably, but one of the elements which differentiate what pathologists are pleased to term acute and chronic diseases:—

"The destruction of a large portion of the hepatic parenchyma, in consequence of chronic atrophy, necessarily entails a diminution of the functional value of the gland, which reacts upon the system at large; and the more so, as a series of derangements in the digestive organs take place at the same time, owing to the obstruction of the portal circulation. In this way other functional complaints accompany atrophy of the liver, and make up the clinical features of this disease. The symptoms are developed slowly and insidiously. First, there are derangements of the gastric and intestinal digestion; loss of appetite, and a feeling of distention, and tightness at the epigastrium; tongue sometimes clean, and at other times furred; accumulations of flatus in the intestinal canal; and pale greyish-yellow- or sometimes moderately brown stools. The bowels are irregular; constipation and diarrhœa alternate; persistent and profuse diarrhœa is frequently (in nine out of eighteen cases) observed, and soon induces exhaustion; in exceptional cases only are the bowels regular. On examining the hepatic region, the dimensions of the organ are found to be reduced in every direction; sometimes no dulness can be made out at all; in most cases, the organ is quite inaccessible to palpation. The spleen is usually unaltered; in a few cases only (in seven out of eighteen cases) is it enlarged.

"Symptoms of defective sanguification and nutrition supervene sooner or later upon these digestive derangements; the patients exhibit a pale cachectic appearance, without any jaundiced tinge; the muscular tissue wastes; and at the same time, as a general rule (in fourteen out of eighteen cases), accumulations of water take place in the peritoneal sac, which are soon followed by general dropsy. The urine is usually pale, and free from bile-pigment,—in a few cases it has presented a peculiar hyacinth-red colour; once only has it been observed to become dirty green, when treated with nitric acid."

It need scarcely be added, that this disease terminates fatally; and the perusal of the author's admirable cases has indeed forced upon us the conviction that chronic atrophy is the final process by which death is brought about, in numerous abdominal diseases. One important observation, referring to diagnosis, we must allude to:—

"Simple atrophy can only be distinguished from that which arises from cirrhosis, when we can ascertain, by means of palpation, whether the surface of the gland is smooth or granular."

The seventh chapter is upon "fatty liver," upon which we are unable to tarry; and proceed to the next, in which the author gives an account of the pigment-liver.

This chapter is written with such marked ability and originality, that we are not surprised at the great attention which has been bestowed upon it by medical writers. The subject is, if not altogether new, at least one in which modern research has greatly added to the confused descriptions of the fathers of medicine. The so-called black bile constituted an important element of the earliest theories of humoral pathology. Galen thought that this substance accumulated in the spleen, as a product of the formation of bile, and that from that locality it gave rise to obstructions of the vessels, enlargements of the abdominal viscera, and dangerous nervous symptoms. We know how great a pathological importance was ascribed to atrabiliary matter by Boerhaave, and Van Swieten. During the first period of the rise of rational medicine, the doctrines of the ancients in this particular were cast aside, but the further progress of minute inquiry has nearly re-habilitated them. For "diseases became known in which black matter produced by decomposition of the blood became developed in the spleen, which black matter passed into the portal vein, and at one time obstructed the hepatic vessels, at another passed through these, and, entering the general circulation, filled up the capillaries of the brain and other organs—morbid processes, in fact, which gave rise to symptoms similar to those which were described by the ancients." Many eminent observers have described a black change of various organs, originating, for the most part, from malarious fevers. Bright gives a drawing of a brain, the cortical substance of which was of a dark colour, "like black lead." It was taken from a man who died of cerebral paralysis, after an attack of fever. It would appear that Meckel, in 1837, was one of the first to ascertain that the dark colour of the organs depended upon an accumulation of pigment in the

blood. A little later, Virchow found numerous pigment-cells in the blood, and in the enlarged spleen of a man who became dropsical after an intermittent fever.

In many individuals, who die from the effects of marsh-poison, the liver presents a steel-grey, or blackish, or not unfrequently a chocolate-colour. This change is produced by pigment-matter accumulated in the vascular apparatus of the gland. In fine sections such accumulation may be seen in the capillary net-work of the portal and hepatic veins; also in the larger branches. The deposits are either uniformly distributed, or in limited regions. Sometimes, owing to the interlobular veins being filled with coloured particles, the brownish-coloured lobules appear surrounded by black margins. In most cases, the arterial system is implicated; the hepatic cells, however, according to the author, remaining free from the dark deposit. As the disease advances, the liver diminishes in volume; and there frequently ensues a true atrophy, provided that the gland does not become infiltrated with colloid matter. Similar abnormal conditions are found in the spleen. When the pigment passes in large quantity into the circulation, the lungs become charged with it, and the brain assumes a remarkable tinge, its capillaries becoming filled with black granules and scales. The kidneys frequently participate in the pigmentary deposit, to a remarkable extent. Grey spots appear in the cortical substance; and, in a few cases, dark lines may be observed in the pyramids following the course of the blood-vessels and uriniferous tubes. The pigment may be detected in the capillaries of the cortical substance, and in the Malpighian bodies; occasionally isolated fragments are observed in the uriniferous tubes. The pigment in other organs and tissues is of subordinate importance. This dark material is brought to the various tissues by the blood. It consists of small rounded or angular granules, true pigment-cells,—some like the colourless corpuscles of the blood, others spindle, or club-shaped, with rounded nuclei. Larger fragments of pigment of irregular form are also observed, sometimes as large as $\frac{1}{20}$th of a line in length. The colour is, for the most part, deep-black, but it may be brown or ochre-coloured, or even reddish-yellow, representing the various stages of transformation of the red pigment of the blood into melanotic matter. Such are, in brief, the post-mortem appearances of melanemic individuals.

In reference to the place and mode of origin of pigment, most observers have agreed to place the seat of formation in the spleen; but, as experience demonstrates that these transformations can take place in any part of the vascular system,

or even external to it, we must pause before *limiting* their seat of production to that organ:—

"There can be no doubt, however, that the largest portion of the pigment is formed in the spleen; that it passes from this organ into the portal vein; and that part of it remains impacted in the liver, while the rest passes through these capillaries, and is carried into the general circulation."

In melanemia, the accumulation of pigment is not so constant in any organ as in the spleen; next to the spleen, in order of frequency, comes the liver. The author has only met with one case where the pigment was present in the liver, while it was absent in the spleen. The question of its manner of formation is still unsolved. Stagnation of the blood in the abdominal glands is the only condition that can be at present predicated in reference to this question.

The effects upon the system of this morbid condition are most formidable; the blood becomes profoundly changed; the circulation in the liver seriously impeded; the bile is secreted in large quantity, and often contains albumen; leucine can always be detected in the hepatic parenchyma. Still more serious obstructions occur in the brain; hence, headache, noises in the ears, deafness, black vision, giddiness, delirium, convulsions, coma. But it is important to recollect that the cerebral disturbances of intermittent fever are not necessarily connected with melanemia: as might be expected, albuminuria is frequent, and complete suppression of the urinary secretion sometimes occurs:—

"The peculiar colour of the skin, which is produced by the quantity of pigment in the blood contained in the vessels of the cutis, is of importance in diagnosis. In the slighter grades of melanemia, it is ash-coloured; in the more intense forms, it is dirty greyish-brown, and sometimes deep yellow-brown. In most cases, a few drops of blood, obtained by scarification of the skin, suffice to show numerous particles of pigment under the microscope."

These derangements are ushered in and accompanied by a fever, which may assume very various forms and types. In most cases, it is an intermittent, but the intermission is usually incomplete; and the three stadia of the paroxysm are seldom very distinctly marked, the rigor stage being often entirely absent. After two or three incomplete intermissions, the fever, in most cases, becomes continued. Some cases prove fatal in a few hours or days, while others are protracted over months.

The author distinguishes four forms: 1, Cases with pre-

dominant head-symptoms; 2, Cases in which the kidneys are pre-eminently implicated; 3, Cases with predominant derangement of the gastro-intestinal tract, and of the appertaining glands, but particularly of the liver; 4, Forms in which the local derangements are not very conspicuous, and characterized by anemia and hydremia, resulting from diseased spleen. There is a valuable collection of cases illustrating these various forms, to which we very earnestly invite attention.

The diagnosis can only be made with certainty by an examination of the blood,—the presence or absence of pigment in which, it must be our main object to determine. To the practised eye, "the peculiar grey, ash-coloured, or greyish-yellow colour of the skin, furnishes an obvious means of diagnosis." The prognosis always remains a matter of doubt; coma and convulsions are, in general, of bad omen.

As soon as we are sure of our diagnosis, large doses of quinia must be administered; simple enlargements of the spleen usually yield to the use of quinia and steel. The congestion of the liver usually disappears spontaneously after the cessation of the fever, or even before this; when it remains stationary, rhubarb, extract of saponaria, with the neutral salts, the ammoniated sesquichloride of iron, with extract of aloes, are recommended. There is no known treatment for the atrophy of the organ. As regards the kidney complication, we find the author making the interesting remark, that the "albuminuria and hematuria which accompany the fever paroxysm, and remit and intermit along with it, *yield best to quinia*, and disappear, for the most part, so soon as the fever ceases." When the abnormal condition persists, extract of cinchona, tannic or gallic acid, and the preparations of iron, are advised. The cerebral disturbances are to be treated on general principles, but we must not neglect the use of quinia.

The final chapter is on "Hyperemia of the liver, and its consequences," to which we can now make no reference.

After this lengthened analysis, our readers will experience no difficulty in gauging the value of this monograph, which indeed presents to us a perfect model of medical research. Nothing can surpass the modesty of the author, but the admirable lucidity of his mode of treating the most difficult subjects. The translator, Dr. Murchison, has fulfilled his task so as to leave nothing to desire. He shows a thorough acquaintance with his author; and, both in his preface and in his most useful notes, has laid his readers under many obligations. We hope much from a physician who has displayed such *thoroughness* in all his undertakings, both as an investigator and as a *littérateur*.

Cellular Pathology, as based upon Physiological and Pathological Histology. Twenty Lectures delivered in the Pathological Institute of Berlin, during the months of February, March, and April, 1858. By Rudolf Virchow, Public Professor in ordinary of Pathological Anatomy, General Pathology, and Therapeutics in the University of Berlin, &c. Translated from the second edition of the original, by Frank Chance, B. A., M. B., Cantab., &c. With notes and numerous emendations, principally from the MS. notes by the Author; and illustrated by 144 engravings on wood. London: Churchill. 1860. 8vo, pp. 511.

It would be a folly to deny that many of our ideas with respect to physiological knowledge are, in the present day, in an extremely unsettled condition; in fact, they may be said to be in that unsatisfactory transition state, in which no definite degree of precision has been arrived at, sufficient at least to command that full amount of confidence which a matter of such importance to the general interests of the human race ought, with every reasonable propriety, to be invested. Doctrines that had been advanced not many years ago as great and incontrovertible truths, and as such universally received, now no longer maintain that high character which then had been so readily conceded to them; and doubts which were at first only faintly whispered with respect to their ambiguous pretensions to public favour, gradually but surely have assumed a louder tone, quite sufficient, at all events, to cast a taint of suspicion on the soundness of the views entertained and propagated by those who were termed the disciples of the new school, whose theories, we will be candid enough to confess, were not always such as could be successfully defended; for they were too often the constructions of a hurried moment, and accordingly presented such a variety of weak points, that a skilful adversary had only to select his place of assault to succeed in his premeditated work of destruction. It was, indeed, hardly to be expected that new and startling doctrines, whose avowed object was to overthrow others that for centuries had been looked up to with reverence and respect, should be exempt from the attacks of the most searching and merciless criticism; and hence became necessary the greater care and caution, to shield with the most consummate skill every avenue of approach of which a watchful assailant might be led to take advantage. Accordingly, no hasty or half-formed conclusion should have been admitted,—no view advanced, that would not bear the strictest and closest ana-

lysis—nothing stated as a fact, that could not be as readily proved to be such; while every sentence, as it was committed to the ordeal of public opinion, should have been so constructed as to endure without flinching the scrutiny which, from its very nature, it seemed to court and defy.

It was not by a series of rapid and unreflecting strides, such as we often are compelled to witness in the present day, that our forefathers wrought out those numerous and truly wonderful discoveries, apparently in the teeth of the most insurmountable difficulties: the sole compensating power which enabled them to succeed in achieving their object, evidently consisting in an untiring patience and steady perseverance, aided by a peculiar concentration of mind, for the time being, on the object of their pursuit. They sifted the subject of inquiry to the very dregs, and left but little to be added by him who followed in their foot-prints, until the rapid progress of modern science placed in the hands of the present generation a multitude of mechanical appliances that were intended evidently to aid, not to supersede, the functions of the intellect. That such a result has not, however, been produced, it would be in vain to deny. We have allowed our better judgment to be led captive by an innovation that as yet wants a firm foundation to rest upon; and hence, those many quicksands that are so continually and treacherously shifting their ground beneath our feet, leaving us too often in a state of extreme peril, uncertainty, and embarrassment.

In former days men were anxious to obtain a distinctive character for the depth of thought, and power of reflection, which they were able to bring to bear on any given subject; and this, in fact, was the scale by which their capacity was universally measured. At present, however, we estimate a man's understanding by no such sensible rule, but foolishly suffer ourselves to be led away by a certain quickness and rapidity of idea, which, when thoroughly examined, is found to be a shadow, and nothing else. The truth would seem to be, that we have no time now for that sober and tedious process of reflection, in the true sense of the word; for our aim is to be considered as an universal genius; and while we thus permit ourselves to dabble in such a multiplicity of subjects, we must as naturally fail in acquiring what might be termed a respectable knowledge in any one of them. The period of our life is by far too short to allow us to indulge in any such visionary expectation of attaining universal excellence; and hence, with many companions in our folly, it is true, we fritter away our

intellect in a pursuit after an ephemeral fame, and sink into the silence of our graves at last, leaving nothing whatever behind us to indicate the position that we once occupied in the world, but a momentary void, which will be almost as immediately filled by some other votary, as anxious to sacrifice at the same shrine of insensate folly.

It is men of this stamp more than any other who are hourly guilty of the most flagrant treason against the true progress of science. They do it, in fact, much more real injury than its most open and avowed opponents; for their ill-directed attempts, illogical reasoning, and lame and impotent hypotheses—for they confess with the greatest candour, that from the multitude of their other avocations, they cannot command sufficient time to handle the subject as its importance might deserve—are positively destructive to a cause, which, in the spirit of charity, we must suppose it was their most earnest wish to advocate and uphold. Did we require a striking example of the truth of what we have just stated, we have only to direct our attention to the scenes that have occurred in our courts of law within the last few years. The lofty pretensions which chemistry once laid claim to,—where are they now? Are they not gone, vanished, and that we fear irrevocably? Are they, we would ask, now considered of the value of a feather's weight in turning the scales of justice one way or the other? Are they not regarded by legal authorities as all but utterly worthless—or, to speak more properly, do they not look upon its noblest deductions merely as an article that is for sale, to be prostituted at any moment to the vilest purposes, as the occasion may require? Dare we assert that this decision, unfounded though it be, in positive fact, has not been arrived at on what may be deemed just and substantial grounds? For it cannot be expected that men totally unacquainted with this particular branch of science will take the trouble of endeavouring to reconcile the apparent contradictions that absolutely do exist—they will look to the fact, and the fact alone; and will as naturally ascribe to the erroneous deductions of chemical science, what ought with greater justice be imputed to the ignorance and want of skill of the contemptible smatterer—the would be-universal genius, whose vanity had led him to suppose that he was capable of using the means at his disposal with the tact and precision of a master in his craft. Try now, when the mischief has been completed, to convince this wretched pretender of the dark obloquy that he has cast on this, the noblest of all sciences. Show him as clearly as the noonday sun, that his reasonings were all so

many fallacies, his views erroneous, and his statements not worth the breath that was expended in delivering them. Think you that you will draw a murmur of regret for what has been done from that mass of animated egotism, that listens with a quiet sneer on his countenance to all that you can urge in your wrath on this occasion. The truth is, that he is at this very instant waiting impatiently for the moment that will enable him to make a similar display of his stock of erudition—limited in one sense certainly, but as truly multifarious in another—on a branch of science the very opposite to the one in which he has so lately shone with lustre so peculiarly his own.

But chemistry has not, however, the honour of standing alone as an example of the discredit that may be brought on a particular pursuit by the errors that must, of necessity, be the offspring of assuming folly, and still more of presumptuous pretension. Let us direct our attention for one moment to the microscope, and see how low it has fallen in general estimation, owing unquestionably to the awkward attempts of a few shallow pretenders, striving to attain a character for proficiency in what, even in the hands of the most expert in the art, must always be beset with a host of difficulties, which nothing but time, patience, and a continuously uninterrupted practice can ever hope to overcome. True it is, that we have always felt, and never took the trouble of concealing this feeling, that the powers of this wonderful instrument of man's ingenuity, constituted as it is at present, have been greatly overrated, and more has been always expected from it than it had the power to accomplish; still, with so many and so important revelations effected by its agency, yet vivid in our memories,—with the explanation of several mysterious facts, hitherto inscrutable, accomplished by the same means, still fresh in the mind's eye,—it would be unjust, as well as unwise, to attribute to the above-mentioned cause alone its gradual declension in public estimation. Take even the most trivial affair in ordinary life, let a diversity of opinion exist, and doubt will assuredly follow. In appealing to one of the most trustworthy of our senses, the microscope did, at first, most certainly acquire a confidence almost unlimited; and no matter how opposed its revelations might be to all our former preconceived impressions, we were still satisfied to receive them with respect, and sacrifice even our own most favourite opinions to its more manifest enunciations. And now arose a host of professional amateurs, each with instrument in hand, and all anxious to parade their limited stock of knowledge before the public gaze,—eager to acquire a character for originality of idea; and unhesi-

tatingly publishing a series of pretended discoveries to the world, that could, in truth, boast no firmer basis of support than the wild and visionary dreams of their own excited imaginations. The theories which they had the folly to advance fell, of course, beneath the storm of criticism with which they were universally assailed; and in their fall, exactly as in the case of its twin-sister, chemistry, barbed the shaft which every fool was ready to seize upon, and hurl against the rational supporter of a branch of science that had failed simply from being abused by those who wanted the intellect to employ its acknowledged powers for the purposes for which it was really destined.

Strange it is, but nevertheless true, that example is utterly thrown away on individuals such as we have alluded to. The first themselves to inveigh with merciless rigour against the error of another—the foremost to point out, and that sometimes with much propriety the irreparable injury that is inflicted on science by crude and ill-judged exhibitions of this nature,—they are themselves equally hasty in falling into the same pitfall, to meet with a fate precisely similar to that which they had been so ready to award to those who had preceded them. The punishment, however, will always fall lightly on men of this stamp; for such is the amount of their overweening egotism and self-esteem, that they will always experience a certain degree of consolation in the reflection, that their want of success ought to be attributed, not to any deficiency on their own part, but rather to the injustice of a jealous world, that sees clearly enough, but refuses to recognise, their high and legitimate claims to well-merited consideration.

We dwell thus strongly on this subject, because we perceive that it is an evil which is latterly acquiring fresh strength; and, if allowed to proceed with the same railroad rapidity, it is difficult to say to what a climax it may yet arrive. Boys hot from the dissecting-room,—whose powers of reflection, from the very nature of the avocation they have been obliged to pursue for so long a period, must be too limited to demand even a passing notice—whose efforts at deduction are too puerile to excite even a compassionate laugh—whose cheek is still graced by the modest down that unkindly refuses to the youthful investigator a single hair wherewithal to deck his object-glass—whose attention never was concentrated on any one single object for five minutes consecutively in their life—do not these embryonic representatives of incipient manhood, in form we mean, not brain, presume to lisp out their doubts of some well-established theory, and in the same breath have the audacity to suggest one of their own as being more rational, and

better calculated to elucidate a grave and intricate subject? Aye, and bearded men, too, whose maturer years ought to argue a fuller amount of wisdom, are not ashamed to follow in the same track, but vie with those speudo-philosophers in their egregious folly,—pursuing, however, a rather more ambitious flight; for their eloquent disquisitions are confined to "cyloblasts, blastemas, plasmas," &c. &c., though the chances are, that there are not more than five out of every hundred that clearly understand the accurate meaning of those technical terms that they are everlastingly, and on every imaginable occasion, forcing into the most ordinary conversation. Nor will they hesitate to volunteer a florid and elaborate description of a particular tissue in a state of disease, though utterly and hopelessly ignorant of the character it presents in a condition of health. In short, they appear to take an insane delight in the effort of exposing their own incorrigible ignorance, quite regardless of the heavy tax they thus impose on the patience and courtesy of their unfortunate auditors, who are compelled to listen in silence to their incessant jargon of gross and incomprehensible absurdities.

In one respect, at least, we may lay claim to a close affinity to the monkey tribe—we mean, in the habit of imitation—not only in small things, but also in great. To conceal a bodily defect, which we instinctively feel will have a tendency to compromise the position which we are anxious to hold in the eyes of the world at large, vanity will not be slow in fabricating a certain style of attire which good sense, in every other way strictly unimpeachable, will as certainly, after a little time, completely lapse into. "Out of the fashion, out of the world," is an old but true remark; and so thoroughly are we convinced that it holds its ground, even to the present day, that we do not entertain the least doubt that, if Sir David Brewster would take it into his head to revert to the amusements of his boyish days, by placing a gold-headed cane between his legs, and so parade the public streets, not one, but a thousand, pseudo-philosophers would be at once similarly mounted, capering and prancing along our pathways, proud of the appearance that they made, and as eagerly seeking for the admiration of every casual passer-by. And so it is with the microscopic mania, which is at present raging like some fierce epidemic—the staid footsteps of wisdom hurrying on in the track that has been imprinted but a few moments before by the lighter tread of folly. Nay, to such a pass has the delusion arrived, that even the busy practitioner—engaged from morning till night in the harassing pursuits of his profession, working literally for his mo-

dicum of daily bread, and the blanket to stretch over his wearied limbs during the midnight hours, with a mind in one continuous vortex of anxiety and disquietude—he, too, if he wishes to hold his ground amongst his brethren, must succumb to the prevailing fashion, and write in living characters his adhesion to a system so contemptibly ridiculous—we say, ridiculous, for surely no rational man can imagine for a moment that the few brief moments that he can snatch from his more active and imperative avocations can, by less than a miracle, be sufficient to unveil to the peculiar quickness of his vision what it would take the professed microscopist weeks, nay, perhaps, months, to unravel satisfactorily. And yet, that such are the melancholy spectacles we are doomed to behold day after day, no one who has devoted the least attention to the subject will presume to deny; and the only excuse that can be urged in extenuation of it is, that it is, unfortunately, the fashion of the times we live in, and, under the circumstances, we are in a manner coerced to keep up to the mark; as a friend of ours once remarked to us, when borrowing a book on microscopic anatomy, to enable him to talk with some degree of fluency on this intricate subject, while, at the same time, he as naively confessed that he had never looked through the tube of a microscope in his life.

"Desipere dulce est in loco," so spoke the most sensible and acute of all the ancient satirists; but we, it would appear, have improved wonderfully on this axiom; for, in season and out of season, we display no symptom of shame in mounting our favourite hobby, and courting a notoriety which it would have shown much more real sense to have avoided altogether. One fact, with respect to the subject under discussion, has often struck us as being very remarkable, and that is, the gross ignorance which is so universally displayed by the profession at large regarding the amount of time and labour that must be expended in microscopic investigations, in connexion with pathology, if the object intended is really to derive any positive advantage from them; and here is another reason why so many shallow pretenders are so continually springing up, while the numbers of the real proficients in the art are comparatively so few. It was in order to correct this evil we have heard that a gentleman, holding a distinguished and well-deserved position in public estimation for years, the physician to one of our largest metropolitan hospitals, has suggested the appointment of a regular microscopical pathologist to each of those invaluable charitable institutions, under the impression that such an individual, acting as a species of accre-

dited *censor morum*, would, in a great degree, tend to modify and control many of those unnatural vagaries that are, at the present moment, poisoning the very life-blood of an incipient branch of science, which, if properly handled, promises to do so much, in time to come, for the cause of humanity. It is needless for us to attempt to describe the pleasure it would afford us, were it possible that the proposed scheme could be thoroughly carried out; but, apart from other difficulties, which are too obvious even to be alluded to, we will merely point to one, which is so tangible in its nature, that it must not be for a moment overlooked. Where are the funds to come from sufficient to compensate the man properly qualified to undertake, and as well calculated to discharge efficiently the duties of such an office, more especially when it is recollected that it is the most valuable hours of his time that will be required from him, aye, and exacted, too, to the last moment? In these degenerate days, the honour of such an appointment will count but little indeed; for, as the rule, the more gifted of mankind are beginning gradually to acquire that amount of wisdom that leads them to decline a gratuitous offer of their brains for fools to banquet upon; for they have at length discovered that, whilst luxuriously indulging their appetite, they are by no means backward in insinuating that it is to them alone (the fools we mean) is due the sole honour and credit of providing the materials for this magnificent feast.

The man of genius, too long a child in simplicity of character, as far as worldly matters are concerned, is at length slowly becoming aware of the precise standard value that ought to be attached to the patronising nod, and ready shake of the hand, with which he may be greeted, according to the current of circumstances. Beneath this specious garb he is instinctively beginning to learn to recognise the identity of the confirmed brain-sucker, who, battening upon the rich intellect of his fellow-man with complacent simper of satisfaction on his face, lisps out his pirated stock of information to the world, with an unblushing assurance, that an honest man might well envy and strive to imitate, but in vain, in a better cause. Not a century has elasped since an individual, with what was then termed the most unjustifiable effrontery, had the boldness to coin out a character for himself from the posthumous ore committed to his charge by a dear friend, and still closer connexion. The voice of public opinion was deservedly raised against this deliberate act of literary chicanery; and the withering scorn of all that was good, all that was honest and upright, was poured in scathing torrents on his devoted head; and yet he had, at least,

the decency to bide his time, and wait until the true owner of what he had so surreptitiously appropriated to his own use had lost the privilege of claiming, with a living voice, what really did belong to him as his own undoubted property. Do the jackals of literature of the present day observe a similar decency in their conduct, and pause until the hand of death has quenched the voice of their victim in everlasting silence? They do no such thing, but filch, and that openly, from the breathing man—aye, and so would they act towards the unfortunate pathological microscopist who would suffer himself to be lured into such a position solely by the imaginary *eclat* attending on such an appointment; for soon, very soon, he would be taught to feel, that while his was all the trouble, toil, and loss of time, others, and those whom he dared not disoblige, would as certainly spring up to lay claim to all the honour and credit that might be the result of his labours and exertions.

We have spoken thus openly, candidly, and perhaps a little strongly, on this subject; for this fashion of pretence and assumed knowledge of abstruse science is becoming absolutely intolerable to every well-regulated mind. Still, we would not wish it to be understood for one moment that we have the slightest intention to find fault with, or decry, what may be commonly termed general microscopic investigations; for in this way we frequently dabble in it ourselves, taking, however, right good care to keep all our imaginary discoveries within the precincts of our own breast, and not foolishly force them on the unwilling ear of an incredulous race, who are always slow in appreciating properly the merits of aspiring talent. We would, therefore, most earnestly advise passionate amateurs in this fascinating amusement—for such always it must be in their hands—to rest satisfied with such specimens as they can procure in abundance from the dealers in such articles, which will at least have this one positive advantage—that of saving them the trouble of amassing a number of nondescript preparations, which, but for the reference on the margin, would never be recognised for what they were intended to represent. Or, again, can they not with good plates by their sides, endeavour to obtain a view of some natural object? And no matter how faint the resemblance may be to the picture before them, surely a glowing imagination, that does not halt at trifles, will be fully competent to fill up many a gaping void, and confer a graceful harmony on the whole exhibition.

All this can do no harm—in fact, it is attended with the positive good of affording a certain relief to the over-wrought

mind, and restoring it to the proper tone for carrying out the more sober details of every-day life; while it as assuredly ceases to be a recreation, and is converted into a positive nuisance, the moment it emerges beyond the privacy of the study, and stalks conspicuously before the eye of the world.

We shall close our homily with the following apt illustration of what we have been attempting, we fear uselessly, however, to impress:—one of the most illustrious statesmen that ever directed and controlled the mighty destinies of Great Britain, was one day detected, when he conceived himself quite free from observation, on hands and knees on the carpet, with a child mounted on his back, to whom he enacted the part of a docile and delighted steed. The feeling that prompted the great and gigantic intellect to descend to such a boyish pastime was one that, under the circumstances, all must admire; for it was a father's heart that induced him to the act, and led him to feel a sympathetic pleasure in the delight which he was thus conferring on the joyous and exulting boy. Photograph him now in this strange position, and suspend him in every print-shop in the metropolis. What is the result? The picture which the most morose ascetic might once have gazed on without, at least, a feeling of displeasure, has now become so positively ludicrous, that even the fool may be pardoned the broader grin with which he salutes it whenever it chances to meet his eye.

In the work of the great and acknowledged German Professor at present lying before us, curiously enough, we find the following passage, corroborating strongly the opinions which have been advanced by us a few sentences back. Speaking of the limited opportunities enjoyed by the busy practitioner for pursuits of this nature, and his want of sufficient time to carry out his investigations effectually, he thus proceeds to say:—

"And for this reason, that I thought I must take it for granted that many busily occupied physicians were not quite familiar with the most recent histological changes, and did not enjoy sufficiently frequent opportunities of examining microscopical objects for themselves. Inasmuch as, however, it is upon such examinations that the most important conclusions are grounded which we now draw, you will pardon me if, disregarding those among you who have a perfect acquaintance with the subject, I behave just as if you all were not completely familiar with the requisite preliminary knowledge."

This soft language, by the bye, is only a happy art of clothing a rather disagreeable subject in a delicate style of words; for we may rest assured that the land of the learned

professor is just as rife in pretension as our own native soil, a fact of which none was better aware at the moment than the speaker himself. Preliminary knowledge, indeed! What a cutting sarcasm these two simple words must have conveyed to the unfortunate auditors that were seated before him; for, with every wish to give them credit as well-educated and thoroughly informed physicians, we are yet strongly inclined to believe that their insight into the elements or principles on which pathology must of a necessity be based, might be measured by the minutest of all homœopathic scales, without the least packing. Nor is he a particle less indulgent a little farther on, where he makes the following remark with respect to the amount of light that is required for microscopic purposes, with the deceptive results that will be produced by its excess,—an observation that might still be farther, and with great justice, extended and applied to several other abuses in the manipulation of this delicate instrument, by which the most absurd and ridiculous effects are frequently produced,—tending unquestionably to confirm an opinion religiously believed, and advocated by an ingenious friend of ours, who upholds, and is not ashamed to defend the doctrine, that all the appearances which we see, or pretend to see, with the high powers we at present possess, are nothing more than minute particles,—the constant constituents of the atmosphere—and that it depends simply on the accidental turn of the movement wheel, which of them may by chance be brought into view. He may be right, or he may be wrong, for aught we know to the contrary, in this age of progress; but the words of the Professor show that something like this may sometimes take place:—

"In the course of the last ten years of the last century there arose, however, a certain degree of reaction against this fibre-theory; and in the school of natural philosophers another element soon attained to honour, though it had its origin in far more speculative views than the former, namely, the *globule*. Whilst some still clung to their fibres, others, as in more recent times Milne-Edwards, thought fit to go so far as to suppose the fibres, in their turn, to be made up of globules ranged in lines. This view was in part attributable to optical illusions in microscopical observation. The objectionable method which prevailed during the whole of the last and a part of the present century—of making observations (with but indifferent instruments) in the full glare of the sun—caused a certain amount of dispersion of light in nearly all microscopical objects, and the impression communicated to the observer was, that he saw nothing else than globules. On the other hand, however, this view corresponded with the ideas common amongst

natural philosophers as to the primary origin of everything endowed with form."

In glancing our eyes over the pages before us, one article, that on the nails, has struck us as being so eminently beautiful, and apparently so true in all its details, that we regard it as a real gem in this casket of science. It follows gracefully the section on the skin; the latter presenting no new feature worthy of remark, and only confirming what we ourselves have always been in the habit of teaching, that the so-called *rete mucosum* is nothing more or less than the deeper and more highly organized layers of the epidermis, and not a distinct structure, as is so generally supposed. Our views, however, with respect to the development and structure of the nail were not of a character so definite and precise; and we were accordingly delighted to receive a lesson on this subjectfrom one who, from his position and language, can be safely relied on. His remarks on its pathological condition are likewise curious, and such as are not unfrequently witnessed in the career of general practice;—

"Very much the same is the case with the nails. On examining the section of a nail, made transversely to the long axis of the finger, we see virtually the same structure as in ordinary skin, only every single indentation of the inferior surface does not correspond to a conical prolongation of the cutis, or papilla, but to a ridge which runs along the entire length of the bed of the nail, and may be compared with the ridges which are to be seen upon the palmar surface of the fingers. Upon these ridges of the bed of the nail are dwarfish, stunted papillæ, and upon them rests the rather cylindrically shaped youngest layer of the rete Malpighii; then follow cells continually increasing in size, until at last the really hard substance comes, which corresponds to the epidermis.

"Nevertheless—to discuss the subject at once, seeing that I shall not again have occasion to mention it—the structure of the nails has been difficult to make out, because they were conceived to be a simple formation. Nearly all the discussions, therefore, which have taken place, have turned upon the question where the matrix of the nail was, and whether the growth of the latter took place from the whole surface or from the little fold into which it is received behind. If we consider the nail with respect to its proper firm substance, its compact *body* (Nagelblatt), this only grows from behind, and is pushed forward over the surface of the so-called *bed of the nail* (Nagelbett), but this in its turn also produces a definite quantity of cellular elements, which are to be regarded as the equivalents of an epidermic layer. On making a section through the middle of a nail, we come, most externally, to the layer of nail which has grown from behind, next to the substance which has

been secreted by the bed of the nail, then to the rete Malpighii, and lastly to the ridges upon which the nail rests.

"Thus the nail lies in a certain measure loose, and can easily move forwards, pushing itself over a moveable substratum, whilst it is kept in place by the ridges with which its bed is beset. When a section is made transversely through a nail, we see, as already mentioned, essentially the same appearance presented as that offered by the skin, only that a long ridge corresponds to every single papilla seen in ordinary sections of the skin; the undermost part of the nail has slight indentations corresponding to these ridges, so that, while gliding along over them, it can execute lateral movements only within certain limits. In this manner, the body of the nail which grows from behind moves forward over a *cushion* of loose epidermic substance in grooves which are provided by the ridges and furrows of the bed of the nail. The uppermost part of the nail, if examined when fresh, is composed of so dense a substance that it is scarcely possible to distinguish individual cells in it without applying reagents, and at many points an appearance is presented like that which we see in cartilage. But by treating it with potash, we can convince ourselves that this substance is composed of nothing but epidermis cells. From this mode of development, you will see how easily intelligible distinctions may be drawn between the different diseases of the nails.

"There are diseases of the bed of the nail which do not affect the growth of its body, but may give rise to changes in its position. When there is a very abundant development of cells in the bed of the nail, the body may be pushed upwards—nay, it sometimes happens that the nail, instead of growing horizontally, shoots perpendicularly upwards, the space underneath being filled with a thick accumulation of the loose cushiony substance (Polstermasse). Thus suppuration may take place in the bed of the nail without the development of its body being thereby impeded. The most singular changes occur in small-pox. When a pock forms upon the bed of the nail, there is nothing to be seen but a yellowish, somewhat uneven spot; but if, on the other hand, it is developed upon the fold, then its traces are left in the shape of a circularly depressed, and, as it were, excavated spot in the body of the nail as it gradually advances, a proof of a loss of substance precisely similar to that which takes place in the epidermis."

We would most strongly recommend the perusal of the article on muscle and muscular fibre to our readers; while we can only express our regret that from its great length, and the consecutive reasoning with which it is treated, we find it impossible to make such extracts as would convey a just idea of it as a whole. For this much, however, we can safely vouch, that it gives a truly excellent exposition of this much-debated subject, superior to any we have as yet met with, and open

to far fewer objections than can be urged against any of those that have hitherto preceded it. The same observations may be applied, and with equal justice, to the description of the development and ultimate formation of bone and its precursor, cartilage. So simple and so clear is the author's language on this intricate subject, that we can follow him without an effort; and find no difficulty, with the assistance of the admirable illustrations, in bringing before the mind's eye the vivid representation of all that he is so anxious to convey. There are, however, in the last paragraph of the fourth lecture, several things so new, and which may yet exercise a very marked influence on an extremely dangerous and stubborn disease, that we are induced to extract it entire for the benefit of the reader.

"These systems of tubes which are found in such a very marked form in bone and the teeth, are to be seen with far less distinctness in the soft structures; and it is chiefly for this reason, I imagine, that the analogy which exists between the soft connective tissues and the hard texture of bone has not been clearly comprehended. These systems are most distinctly seen in parts which are more of a cartilaginous nature, as, for example, in fibro-cartilage. But it is a fact of great significance that we find a series of transitional forms between cartilage and the other connective tissues, in which the same conditions are constantly repeated. In the first place, parts which chemically belong to the class of cartilages, for example, the cornea which yields chondrine when boiled, although nobody regards it as real cartilage. But more striking is the arrangement in those parts in which the external appearance speaks in favour of a cartilaginous nature, but the chemical properties do not correspond, as, for example, in the semi-lunar cartilages (Bandscheiben) of the knee-joint, which are interposed between the femur and tibia for the purpose of protecting the articular cartilage from too violent contact. These parts, which even now are generally described as cartilage, yield no chondrine on boiling, but gelatine; and yet in this hard connective tissue, we meet with the same system of anastomosing corpuscles that prevails in the cornea and in fibro-cartilage, and it is displayed with unusual distinctness and clearness. Vessels are almost entirely wanting in these cartilages, but in exchange they contain a system of tubes of rare beauty. On making a section, we see that the whole is in the first place mapped out into large divisions, exactly like a tendon; these are subdivided into smaller ones, and these are pervaded by a fine, stellate system of tubes, or, if you will, of cells, inasmuch as the notion of a tube and that of a cell here quite coincide. The networks of cells which here form the system of tubes, terminate externally in the septa bounding the individual divisions, and we here see in close proximity considerable collections of spindle-shaped cells. In these cartilages, too, the whole mass of tissue is only connected by its exterior with

the circulatory system; everything that penetrates into the interior must pass by a very circuitous route through a system of canals with numerous anastomoses, and the nutrition of the internal parts is altogether dependent upon this mode of conveyance. The semilunar cartilages are structures of considerable extent and great density, and, as they are entirely dependent for their nutrition upon this ultimate, minute system of cells, we have in them, much more than in cartilage, to deal with such an arrangement for the supply of nutritive juices, as cannot be under the direct control of the vessels.

"For the sake of elucidation, I will merely add that the ultimate elements are seen to consist of very delicate cells, which are prolonged into fine filaments, that in their turn ramify, and look, when cut across, like small points in which a clear centre can be recognised. The filaments can ultimately be very distinctly traced back to the common cell, just as in bone. They are extremely fine tubes which are intimately connected with one another, only that here they are in certain spots collected into large groups, by means of which the conveyance of the nutritive juice is principally effected, and that the intercellular substance in no instance becomes infiltrated with lime, but always preserves its character as connective tissue."

In the space to which we are compelled to confine ourselves, it is utterly impossible to notice, as we would wish, many subjects of a character strikingly novel, and certainly very much at variance with the opinions hitherto looked upon as firmly established in every respect. In this category we might class the several articles on the blood, lymph, &c., with the changes that so frequently are well known to occur in them, constituting a variety of diseases that are recognised under the name of pyemia, leuckemia, phlebitis, emboli, &c., &c. Every well-informed physician is, of course, perfectly aware of the symptoms that are usually found to identify each of these several lesions, and which will occasionally start up in his path in the course of his practice, but we are much inclined to doubt whether he as accurately understands the true pathological principles of their nature. To such as are naturally inquisitive in such matters—and what properly educated man would confess the reverse?—we would most earnestly recommend the perusal of the several lectures devoted to these topics, which he will find handled in a style so masterly and so explicit, and yet so free from every vestige of narrow-minded pedantry, that we are convinced he will feel exactly as we did when we had terminated, astonished that the simple views here laid down had not previously occurred to the mind of any preceding pathologist. And herein we conceive lies the striking excellence of the

whole work, that we are not called on to believe what is contrary to our senses, opposed to our reason, and at variance with our better judgment—on the contrary, we are rather invited to call into operation the functions of our own thinking power, the gifted lecturer himself merely suggesting the subject for thought, and leading us insensibly on, until he has conducted us to the climax, almost imperceptibly, indeed, which he had long before anticipated.

We will close our extracts from this volume by one from the last lecture, which, as giving the opinions of the author, with respect to the microscopical and pathological nature of tubercle, is deserving of an especial notice; for who has not had reason to mourn from the ravages of this inexplicable disease? and where the physician, so proud of his own attainments, who has not felt the blush of shame mantling his cheek, as the slow and insidious step of this deadly affection stole silently, but slowly, on, and he was powerless in checking its onward progress? Before this unsparing and relentless foe, that mocks its intended victim, while its remorseless gripe is gradually tightening on the well-springs of life, how lowly we fall in our own estimation, wringing from us the sincere, but bitter confession that, in this case, all our art and all our knowledge stand forth like pigmy dwarfs against such a gigantic and invincible antagonist. The fate of the French medical student, who, made aware of this incurable affection under which he was labouring, dared to anticipate by more speedy means the event which he knew must be his lot, though by a process slower, indeed, but equally as sure, has branded in characters that never can be effaced our utter impotency to deal with this destructive malady successfully in the present condition of our knowledge. We ought, therefore, to hail as it deserves, every attempt that may be made to give us a proper insight into this hitherto unfathomable secret, as it may thus conduct us perhaps to a more rational mode of encountering an enemy who has hitherto proved so totally unassailable in every respect:—

"It may, at least according to what I consider to be the correct view of the matter, certainly be said, that the greatest part of whatever in the course of tuberculosis does not appear in the form of granules, is an inspissated inflammatory product, and has at any rate no direct relation to tubercle. But by the side of these inflammatory products, or also independently of them, we find a peculiar structure [the knot, granule] which, if they are to be regarded as real tubercle, would no longer be included in their ordinary classification; and it is certainly an extremely characteristic circumstance that in France, where the terminology of Lebert has become the pre-

vailing one, and the *corpuscules tuberculeux* are wont to be regarded as the necessary accompaniments of tuberculosis—bodies, concerning the tuberculous nature of which there can be no doubt, have quite recently been set down as something altogether peculiar, and which had hitherto remained undescribed. For one of the best, nay, perhaps the best, micrographer France possesses, Robin, has, in his examinations of cases of tubercular meningitis, deemed it impossible to regard the little granules in the arachnoid [pia mater] which everybody looks upon as tubercles, as being really tubercles, because the dogma now prevails in France that the tubercle consists of solid non-cellular corpuscles, and in the tubercles of the cerebral membrane cells in a state of perfect preservation, are met with. To such curious aberrations does this track lead, that one ends by being unable to find a name for real tubercle, because so many accidental objects have been confounded with it, that what was sought for, or even what had been found and was already grasped, has in consequence of the attention of observers being diverted by these objects, been allowed to slip out of one's hand again. I am of opinion that a tubercle is a granule, or a knot, and that this knot constitutes a new formation, and indeed one which, from the time of its earliest development, is necessarily of a cellular nature, and, generally, just like all other new formations, has its origin in connective tissue, and which, when it has reached a certain degree of development, constitutes a minute knot within this tissue, that, when it is at the surface, projects in the form of a little protuberance, and consists throughout its whole mass of small uni- or multi-nuclear cells. What especially characterizes this formation is the circumstance, that it is extremely rich in nuclei, so that when it is examined as it lies imbedded in the tissue which invests it, at the first glance there seems to be scarcely anything else than nuclei. But upon isolating the constituents of the mass, either very small cells provided with one nucleus are obtained—and these are often so small that the membrane closely invests the nucleus—or larger cells with a manifold division of the nuclei, so that from twelve to twenty-four or thirty are contained in one cell, in which case, however, the nuclei are always small, and have a homogeneous and somewhat shining appearance.

"This structure which in its development is comparatively most nearly related to pus, inasmuch as it has the smallest nuclei and relatively the smallest cells, is distinguished from all the more highly organized forms of cancer, cancroid and sarcoma, by the circumstance, that these contain large, voluminous, nay often gigantic corpuscles with highly developed nuclei and nucleoli. Tubercle, on the contrary, is always a pitiful production, a new-formation from its very outset miserable. From its very commencement it is, like other new-formations, not unfrequently pervaded by vessels, but when it enlarges, its many little cells throng so closely together, that the vessels gradually become completely impervious and only the larger ones, which merely traverse the tubercle, remain

intact. Generally fatty degeneration sets in very early in the centre of the knot (granule), where the oldest cells lie, but usually does not become complete. Then every trace of fluid disappears, the corpuscles begin to shrivel, the centre becomes yellow and opaque, and a yellowish spot is seen in the middle of the grey translucent granule. This is the commencement of the *cheesy metamorphosis* which subsequently characterizes the tubercle. This change advances from cell to cell farther and farther outwards, and it not unfrequently happens that the whole granule is gradually involved in it.

"Now, the reason why I think that the name of tubercle must be specially retained for this formation, as being extremely characteristic of it, is this—that the tubercle-granule never attains any considerable size, and that a tuber never arises out of it. Those which are wont to be termed large tubercles, and attain the size of a walnut, or a Borsdorf apple, as for example in the brain—those are not simple tubercles. You will generally find the tubercles in the brain described as being solitary, but they are not simple bodies; every such mass (tuber) which is as large as an apple, or even not larger than a walnut, contains many thousands of tubercles; it is quite a nest of them which enlarges, not by the growth of the original focus (granule), but rather by the continual formation and adjunction of new foci (granules) at its circumference. If we examine one of these perfectly yellowish white, dry, cheesy tubera, we find immediately surrounding it a soft, vascular layer which marks it off from the adjoining cerebral substance—a closely investing areola of connective tissue and vessels. In this layer lie the small, young granules, now in greater, now in less, number. They establish themselves externally [to the previously existing ones], and the large tuber grows by the continual apposition of new granules (tubercles), of which every one singly becomes cheesy; the whole mass, therefore, cannot in its entirety be regarded as a simple tubercle. The tubercles themselves remain really minute, or as we are wont to say, *miliary*. Even when on the pleura, by the side of quite small granules, large yellow plates, looking as if they were deposited upon the surface, are met with; these too are not simple tubercles, but masses composed of a large aggregate of originally separate granules.

"Here, you see, form and nature are in reality inseparably connected. The form is produced by the growth of the tubercle from single cells of connective tissue, by the degenerative proliferation of single groups of connective-tissue corpuscles. Thus, without more ado, it appears at once in the shape of a granule. As soon as it has once attained a certain size, as soon as the generations of new corpuscles which develop themselves out of the old histological elements by a continual succession of divisions, at last lie so close to one another, as to cause a mutual arrest of development, gradually to induce the disappearance of the vessels of the tubercle, and thereby to cut off their own supplies, then they begin to break up, they

die away, and nothing remains behind but débris—shrunken, disintegrated, cheesy material.

"The cheesy transformation is the regular termination of tubercle; but, on the one hand, it is not the necessary one, inasmuch as there are rare cases, in which tubercles, in consequence of their undergoing a complete fatty metamorphosis, become capable of reabsorption; and, on the other hand, the same cheesy metamorphosis befalls other kinds of cellular new-formations; for pus may become cheesy, and likewise cancer and sarcoma. This metamorphosis, therefore, being common to more than one formation, cannot well be set down as a criterion for the diagnosis of any particular structure, such as tubercle; on the contrary, there are certain stages in its retrograde metamorphosis, where one cannot help confessing that it is not always possible to come to a decision. If a lung be laid before you with cheesy masses scattered through it, and you are asked if that be tubercle or no, you will frequently be unable to say with certainty what the individual masses originally were. There are periods in the course of development when that which is inflammatory and that which is tuberculous can with precision be distinguished from one another; but, at last, there comes a time, when both products become confounded, and when, if one does not know how the whole arose, no opinion can any longer be formed as to what its nature is. In the midst of cancerous masses also cheesy spots occur which look exactly like tubercles. I have demonstrated that it is by the gradual transformation of the elements of cancer that this cheesy matter is produced. But if we did not positively know from the history of their development that cancer-cells disintegrate step by step, and that no tubercles form in the middle of cancer, we should in many cases be altogether unable to arrive at any decision from merely examining the specimen.

"If these difficulties be surmounted which lie in the external appearance of the formation, and lead the observer astray not only when he considers its grosser features, but also when he investigates its more intimate composition, there remains nothing else to assist us in coming to a right conclusion than the investigation of the type of development displayed by the individual new-formations during the stages of their actual development, not during those of their retrograde metamorphosis. The nature of tubercle cannot be studied after the period when it becomes cheesy, for from that time its history is identical with the history of pus which is become cheesy; an earlier period must be chosen when it is really engaged in proliferation. So in the case of other formations, that period must be studied which is comprised between their origin and their culminating point, and we must see with what normal physiological types they agree. Then it is, I think, certainly possible for us to arrive at a just conclusion with the aid of the simple principles of histological classification, which I have already propounded to you. Heterologous tissues also have physiological types."

In bringing his task to a conclusion, it is really surprising with what a species of fond regret a reviewer, more especially where he has found but little to awaken the bitterness of damnatory criticism, finds himself compelled by circumstances to bid farewell to his author. The close attention which, if he is sincerely desirous of performing his task conscientiously, he must concentrate on the subject as necessarily creates a kind of consentaneity of idea between them, a feeling that gradually becomes so strong that we experience a certain natural reluctance in bursting too abruptly the intimate bond of connection that has thus been insensibly produced. The opinion which too universally prevails, and which we also fear is too generally believed, that men devoted to science are always too ready to sap the reputation of a rival in the same path of literature, whenever a favourable opportunity may present istelf, is a gross and wicked libel, as utterly devoid of truth as it is indicative of the bad taste of those who can stoop to the propagation of a scandal so completely without any real foundation. The generous mind sees in the true follower and able delineator of purely scientific truths, simply the living impersonation of science itself; and its ardent votary bends the knee, without a single feeling of jealousy, at the same shrine with his fellow-worshipper—a community of feeling engendering a mutual respect, sufficient to annihilate altogether those contemptible exhibitions of petty rivalry which manifest themselves but too frequently in the other ordinary paths of life. The realms of knowledge, in their wide and unmeasurable expanse, present so many regions yet untouched, that there is abundant scope for all, without any danger of interference one with the other; and of this the work before us presents a striking example—it has opened a new era in pathological science, as depending upon microscopical revelations; and as such it must be carefully and universally studied by those who are anxious to maintain their proper ground in the estimation of the world, as being up to the standard of information of the present day. To the able translator, who has so faithfully accomplished the difficult task of transferring to a language not his own, not only the words, but likewise the exact sense and peculiar style of their author, our gratitude is especially due; for without his aid, this great monument of patient labour, accurate research, and stupendous learning, must have remained comparatively a dead letter, accessible to the few only, and not, as he has made it by his exertions, the clear and undisputed appanage of the many.

Clinique Médicale sur les Maladies des Femmes. Par M. G Bernutz, et M. E. Goupil. Paris: Chamerot, 1860. 8vo Vol. I., pp. 691.

The work before us, though bearing the name of "Medical Clinique on Diseases of Females," is not intended, as Dr. Bernutz takes care to inform us in the very first lines of his preface, to treat of *all* diseases incident to females, but of those only "the existence and symptomatology of which is beyond all doubt." We have as yet received but the first volume of the work, and in it are treated only certain internal hemorrhages, viz., those depending on *retention of the menses*, *peri-uterine hematocele proper*, and on *intra-pelvic hemorrhage, depending on extra-uterine fœtation.* The principal interest of the work depends on the number of carefully collected and diligently studied cases of that form of hemorrhage to which the name of "peri-uterine hematocele" has been given—a form of disease which, though for several years both recognised and studied on the Continent, has till very lately been completely ignored by us at home. We quote the words of our able contributor, Dr. M'Clintock, who lately gave us an interesting account of a case of peri-uterine hematocele. He says:—"Dr. West and Dr. Tilt are the only English writers I know of who have written upon this disease, or met with examples of it; nevertheless, for the last ten years it has been the subject of many essays and memoirs by French physicians and surgeons, especially, MM. Huguier, Nelaton, Trousseau, Nonat, Laugier, Oulmont, Puech, Bernutz, &c., &c." This last is the author of the work at present before us, with whom is associated Dr. Goupil; but who, as we learn from the preface, only contributes the article on "uterine deviations." To the above list of authors who have treated of this disease, we can add the name of Becquerel, who wrote about it in his "Traité Clinique des Maladies de l'Uterus, et de ses Annexes," in 1859; and of Voisin, who, in 1860, wrote "De l'Hématocele Rétro-Uterine," &c., &c.

Dr. Bernutz divides into two well-marked classes all cases of menstrual retention, viz., such as depend on difficulty of *secretion*, and such as depend on difficulty of *excretion*;—he dwells on the great importance of carefully distinguishing the one from the other, as an error in the diagnosis would lead to still greater errors in the treatment.

After a rapid review of the theory of ovulation as the cause of the catamenial discharge, he recapitulates in a few words as follows:—"The menstrual excretion, in order to be effected

normally, requires a concurrence of several distinct processes; for instance, congestion, application of the Fallopian tube to the ovary, dilatation of the orifices, especially that of the neck of the womb; and if any of these should fail, either entirely or in part, such interruption of the function may result as may be productive of more or less serious complications, the cause of which must be fully recognised before an adequate and rational treatment can be applied." Peri-uterine hematocele is probably not a very rare disease; and the author is inclined to think that the cause of its having been so long unobserved is due to the serious peritonitis which generally accompanies all effusions of blood into the peritoneal cavity; and which on the one hand monopolized the attention of the physician, whilst such hemorrhages as occurred externally to the peritoneum were erroneously considered as cases of metritis, or of phlegmons of the broad ligaments, and of the iliac fossæ. Having devoted only eight pages to these considerations, the author at once proceeds to the description of the several cases which form the body of the work, and which are full of interest; and we cannot do better than give a short epitome of some of the more interesting.

Case 1.—August 13, 1844. F——, aged 40, of delicate constitution, though she never had a positive illness. Menstruated at 17, and continued regular, until a short time since, except during her pregnancies, of which she had seven. During her first five pregnancies, she got herself bled; and on each occasion she had a premature confinement, between the sixth and seventh month. During her last two pregnancies she abstained from getting herself bled, and the confinement did not occur before its normal term. About four months since, after some hard work, during which she had to carry heavy loads a long distance, the menstrual period did not occur at the expected time; and in a few days the patient was seized with violent pains in the abdomen, similar to the pains of labour. After a few days, these pains diminished, and continued in a mitigated form till the next menstrual period, when a violent exacerbation occurred, but unaccompanied by any catamenial discharge. Together with the pain, she suffered much from nausea and vomiting, and had considerable fever also. The treatment consisted in leeches to the anus, blisters to both hypochondria, and a hot bath daily. About three weeks later, she was attacked by very violent pains at the time she was in the bath, which were followed by the passage of a membranous clot, followed by a slight and pallid discharge. After this occurrence, the patient slightly improved, until the

16th of August, when, having suffered another exacerbation, she sought for admission into hospital. Her condition was then as follows : she was emaciated and pale; the expression of her countenance was anxious; appetite, nil; no nausea, no vomiting; bowels, free; urine, normal. No pain in the lumbar region on pressure; abdomen clearly protuberant inferiorly and laterally into the iliac fossæ, extremely tender on pressure; no perceptible fluctuation; digital exploration *per vaginam* increased the pains excessively. The treatment consisted in leeching the abdomen, the use of baths and poultices, low diet (diète), and lemonade. She died on the 30th, two weeks after her admission. Her body was examined, forty hours after death, when the following condition of parts was discovered in the abdomen : the stomach, liver, intestines, &c., where in contact with the peritoneum, were found slightly adherent; and, in the depending portions of the cavity, there were about three pints of a brick-red liquid; the parietal peritoneum, of a slaty grey, was studded with red maculæ in its upper portion, whilst in its inferior half it became of a deep indigo, marbled with black.

The external surface of the intestines was similarly coloured, and the inferior six inches of the small intestine, together with the cœcum, were completely melanotic. At about the level of the superior strait, the following parts were found solidly matted together, forming a perfect division between the upper and lower regions of the peritoneal cavity, viz.: the uterus, bladder, two ovarian tumours, the sygmoid flexure of the colon, the inferior portion of the small intestine, the cœcum, and the vermiform appendix. The circumscribed cavity, of which the above-named viscera formed the roof, was lined by a much degenerated peritoneum, covered extensively with villous exudations of lymph; it was incompletely divided into two unequal cavities by an antero-posterior sort of membranous expansion, the right portion of which contained a quantity of brick-red liquid, while the left enclosed a large and *firm* clot, having the shape of a truncated cone. The tumours, which we have perhaps improperly called ovarian, were of unequal size; the one to the right, not larger than an egg, of a deep red colour, and studded with villosities, was found, on examination, to be formed by the fringed extremity of the right fallopian tube, which so firmly adhered to its corresponding ovary, that, in attempting to separate the two, the substance of the tube itself gave way before it could be separated from its adhesions; the cavity of the tumour was filled with altered blood, pus, and clots, while the intra-uterine portion of the

tube was full of crimson blood. The tumour on the left side was considerably larger, and differently constituted—its boundaries being formed not only by the fringed extremity of the Fallopian tube and its corresponding ovary, but also, in great part, by a portion of parietal peritoneum, together with adventitious membranes connecting the two. The uterus was simply hypertrophied, especially at its neck, and its cavity dilated. No communication could be detected between the cavities of the tumours and that of the pelvi-peritoneal cyst.

The author concludes from the above examination as follows:—

"The hypertrophy of the neck of the uterus, acting in this case similarly to an enlarged prostate in man, interfered with the free exit of the discharge at each menstrual period, producing a high degree of congestion of the generative organs in the first instance; distention of the cavities, and formation of the hypogastric tumours, in the second; whilst at the third and fourth periods, there were further distention and laceration of some of the false membranes, by means of which the blood found its way into the cavity of the peritoneum, where it excited more than one attack of inflammation, which eventually caused the death of the patient."

The study of the foregoing case induced the author to reject as *independent idiopathic affections* the following diseases viz., *uterine congestions: metreites; phlegmons of the broad ligaments; inflammations of the ovaries and the peritoneum, fleshy moles; and dropsies, either of the uterus, Fallopian tubes, or ovaries;* and to refer them all to one common source, viz., retention of the menstrual flux.

We have no doubt that many of these diseases may arise, and do arise, from retained menstrual flux; but we cannot agree with the author that *all* do; for instance, *fleshy moles* (*môles charnues*), are the result of some irregular, vitiated, or arrested form of conception, as is evident from their partial organization, and the heterogeneous substances—hairs, bones, teeth, &c., &c., found generally in them. Had the author spoken of "*Môles Hydatiques*," instead of "*Môles Charnues*," there he would have been on more debateable ground, as opinions are considerably divided upon that subject. Ruysch, Sir C. M. Clarke, and Evory Kennedy believe that they may occur in virgins; while Denman, Capuron, Boivin, and the late Dr. Montgomery, consider them to be always the result of impregnation.

Having referred all the above diseases to retained menstrual flux, the author proceeds to enumerate the different

causes which may produce that retention; and classes them under eight heads:—

1. Imperforation of the vulvo-uterine passage, congenital or accidental, previous to puberty, occasioned by cicatrices, &c., &c.

2. Obliteration as above, but posteriorly to puberty.

3. Partial obliteration of the vagina, or the neck of the womb, either congenital or accidental.

4. Hypertrophy of the neck of the womb.

5. Accidental occlusion of the neck of the womb by polipi, adventitious membranes, &c., &c.

6. Deviations and flexions of the womb, whatever their origin.

7. Spasmodic contraction of the neck of the womb.

8. Abnormal functional condition of the Fallopian tube, either congenital or acquired.

We have given this classification *in extenso*, out of regard to the author,—not, however, that we approve of it; for many of the distinctions are, in fact, without a difference, and serve only the double purpose of increasing the volume, and mystifying the reader. Not content, however, with this arrangement, he further subdivides the first class into four sub-classes; but we cannot follow him in these minutiæ, and prefer passing on to some of the interesting cases he records—all full of instruction, and many painfully so, from the reckless mode of treatment adopted.

Case 2. A. B., aged 22, never had menstruated; since the age of 13, had suffered every month, for three or four days, paroxysms of pain, which returned with unfailing regularity. For some years back, the abdomen has been considerably enlarged; and, although her general health continued good, yet the monthly attacks of pain continued to recur. A vaginal examination revealed the presence of a membrane completely stretched across the vagina, behind which it was clear that fluid was existing. This membrane was crucially incised; a quantity of blood came away, as also some pints of a black inodorous fluid, without clots. For three days the menstrual discharge continued, without producing any unpleasant symptoms; the abdomen diminished in size, and the patient appeared to be recovering; when, on the fifth day, she was attacked by peritonitis, and died nine days after the operation.

Sectio cadaveris.—Peritoneum inflamed, intestines covered with lymph, and bathed in sero-purulent fluid, uterus larger than usual, os tincœ patent, vagina enormously distended; the uterine half of the Fallopian tubes was enlarged to the size of

a man's little finger, and distended with black blood; the ovarian extremity of the tubes was so attenuated, that the slightest pressure would cause the blood to ooze out.

Case 3 is similar in its history to the previous one. Relief was given by puncturing the imperforate hymen, which was the cause of all the mischief; everything went on well till the fourth day, when she was seized with peritonitis, and died on the fifth. At the post-mortem examination, in addition to the usual appearances of peritonitis, there was found a quantity of a tarry liquid, similar to what had escaped from the vaginal puncture during life; the Fallopian tubes and ovaries were enlarged to such a degree as to form cysts capable of holding a pint each, and in their walls were ulcerations, through which the tarry fluid had found its way into the cavity of the peritoneum.

Case 5 is taken from the London Medical Gazette, and related there by Sir B. Brodie; it was pretty similar to the preceding one; was treated by puncture of the occluding membrane. Death occurred a few days after from peritonitis. On examination of the body after death, a large quantity of menstrual blood was found in the peritoneal cavity; but no laceration whatever, either in the uterus or the tubes, by which the blood could have escaped.

Case 6. E. D., aged 26, had enjoyed excellent health up to the twentieth year of her age, when she was attacked by violent abdominal pains, which recurred monthly.

After some time, these recurring pains having become intolerable, and the patient having lost her previous good health, she consented to a local examination, when it was found that the vagina was closed at about its upper-third, by a transverse membrane. It was incised with a narrow knife, carried forward on the index of the left hand, when a quantity of dark grumous blood was evacuated. The abdomen, which previously had been tumid; became flat; and the patient did not complain of uneasiness until after a few hours, when she was seized with convulsions; the abdomen again became tense; she was attacked by peritonitis, and in two days was dead. At the post-mortem examination, sero-purulent fluid, and putrefied blood, were found in the cavity of the peritoneum; the tubes were closely adherent to the ovaries, and greatly dilated; one of them presented a considerable *rent*, through which a semi-putrefied blood was freely passing into the peritoneal cavity.

Case 7. Similar to the above, only that death occurred naturally; no operation having been performed.

Case 8. Similar to Case 6; operation, and death.

Case 14 is interesting, because we cannot help thinking that, if some treatment had been adopted, at the same time that the patient had been supported by food and stimulants, her life might have been saved, as she appears to have sunk from exhaustion.

Case 22. Menstrual retention, caused by cancerous growths of the neck of the womb; after some time, the womb increased considerably in size, when, after five days of violent pains, like labour pains, a large clot was expelled, the uterus diminished, and the pains ceased.

Case 23. Menstrual retention, due to the presence of a polypus; intense pains at each recurring period; removal of the polypus; recovery.

Case 27. Taken from the "London Medical Gazette," of 1846, is related by Oldham, and considered by him as a case of *pseudo-membranous dysmenorrhœa*. The author does not take the same view as Oldham, and does not believe in the *ovarian influence* to which Oldham says the disease is due. He entirely agrees with Tilt, whose words he quotes from the "Lancet," of 1853, and further adds:—"The existence of the lesions indicated by Mr. Tilt, in the preceding autopsies, completely annihilate all Mr. Oldham's arguments, which are solely based on the microscopic examination of the dysmenorrhœal membrane."

Case 28 is interesting from its cause principally. Julia Dufour, of dark complexion and high colour, 37 years old, was always regular since the age of 14, when she first menstruated; has no children; is not happy with her husband. Being of a violent temper, she often gives way to fits of anger, which have often been followed by excessive menorrhagia. On one occasion, however, her fit of rage coming on during a menstrual period, it caused its suppression, which was followed by very serious complications, including the appearance of a considerable hypogastric tumour. She was treated with ergot; and, after a time, having expelled a quantity of clotted semi-putrefied blood, she recovered.

Case 31 is of considerable interest, as, during life, it was supposed to have been one of peri-uterine hematocele. A. W., aged 28, fair and slight, has never menstruated; up to the age of 24, she never even suffered from any excitement of the genital organs. She has a sister who menstruates regularly, but always with excessive pain. At the age of 24, A. W. was attacked by violent pains in the back and in the lower belly, which, after a few days, ceased, but returned again a month later; and continued thus returning month after month, with

unabated violence, for the space of four years. At the end of this time she came into hospital, when her condition was noted as follows:—abdomen tumid, resonant under percussion in its upper half; absolutely dull inferiorly. By careful palpation, a large, rounded mass can be felt in the right iliac fossa. The index finger of the right hand being introduced into the vagina, it meets with the lower surface of a tumour, about the size of the head of a fœtus, apparently impacted in the upper strait. This tumour is hard, and painful on pressure; nowhere can fluctuation be detected, neither can any arterial throb be felt over its surface. After a few days, a slight discharge made its appearance, accompanied by a few clots of blood, at the same time that she was seized by fever, accompanied by violent delirium, which, however, subsided on the application of twenty leeches to the abdomen. She had now been a month in hospital; and her sufferings being on the increase, M. Huguier, under whose care she was, determined upon performing an operation for her relief; this was to consist, first, of a puncture into the tumour through the lower part of the abdomen, which was then to be continued, by means of a long, curved trocar, through the inferior portion of the tumour, into the vagina. The first portion of the operation being completed, exit was given to a quantity of thick, chocolate-brown liquid, full of minute clots of blood. Before, however, proceeding to the second step of the operation, M. H. thought well of introducing a canula into the bladder, when, by exploring both by the vagina and the rectum everywhere, the finger encountered the canula in front of the tumour, as if, in fact, the rectum and the bladder were spread out over it, and consequently the second step was not proceeded with. Immediately after the operation, the patient got a prolonged rigor, and afterwards complained of agonizing pains. She was treated with mercurial inunctions, leeches, and croton oil frictions on the thighs.

Five days later, she being somewhat recovered, and the tumour having again filled up, M. Huguier made a further examination, both by the vagina and by the rectum, when, having ascertained that for a space of about two centimetres, the tumour was uncovered by either rectum or bladder, he determined to puncture it through that space, and gave exit to a large quantity of extremely fetid greyish-coloured sanies, accompanied by puffs of fetid gases; a gum-elastic tube was introduced, and a solution of iodine injected. The patient was instantly seized with vomiting, violent pains, facies Hippocratica, and matters appeared to be drawing to a conclusion. 20

leeches to the abdomen, and some tincture of aconite, relieved these symptoms, and on the following day the patient was better. Thus she continued, one day improving, and retrograding the next, for sixteen days, when she was attacked by phlebitis, and died four days later. The body being examined, the intestines were found glued together by lymph, and bathed in dark fetid pus. The liver was immensely enlarged, while the rectum, uterus, broad ligaments, and bladder, were covered with a thick layer of pus and false membranes. The left Fallopian tube was found dilated in its entire length; the fringed extremity closed, forming with the tube a sort of club-shaped tumour, of the size of a pigeon's egg. On cutting into it, some chocolate-brown fluid made its escape. The left ovary was enlarged and softened. On the right side, all the pelvic cavity was filled by an irregular anfractuous sac, the inferior portion of which was gangrenous, and of a dark-grey colour; at the most depending part could be seen an ulcerated opening (formed by the trocar), which, passing between the rectum and the bladder, communicated with the vagina. The uterus healthy, though adherent by strong false membranes to the bladder. The right ovary and right Fallopian tube could not be recognized.

The retention of the menstrual discharge often produces, as we have seen, very serious accidents; and in the long run, may give rise to such serious complications as to imperil the life of the patient. We have seen that at first the symptoms are slight, being chiefly pain in the lower belly, accompanied by a sense of weight in the pelvic basin. At the next menstrual period, however, these pains are of a more decided character; and at the next period again, some additional symptoms generally are superadded, viz., pains in the lumbar and sacral regions, with pressure, and bearing-down, simulating in a great degree the pains of labour. If no relief is given by affording exit to the pent-up secretion, either by the unaided efforts of nature, or through surgical assistance, the disease will further progress; and at future menstrual periods, feverishness will be superadded to the previous symptoms; tumour on tumour will probably arise in the iliac fossæ; and if still no external exit is provided, the menstrual fluid, having dilated the tubes and the fringed extremities beyond their power of resistance, will find its way into the abdomen, and thus give rise to peri-uterine hematocele, of the encysted variety, should the flow of blood into the peritoneal cavity be slight and gradual; and of the unencysted variety, should it be sudden and abundant.

Should a case present itself where the catamenia have evi-

dently been trying to establish themselves at several periods, but where, owing to an occlusion of the vagina, it is physically impossible for them to find an exit, is it prudent for the surgeon to interfere, and how? We cannot fail to be struck at the number of deaths recorded in this work after surgical operations; and *that*, even when apparently the most trivial puncture was resorted to only. Dr. Bernutz is of opinion that when surgical interference is determined on, it must not be adopted too late. He thinks that hemorrhage into the peritoneal cavity has often been *the consequence of the operation itself*, from its having startled the uterus into more vigorous contraction, at a time when both it and its tubes were gorged with blood. We would suppose also that the nature of the occlusion would have the greatest influence upon the likelihood of success in operating. For instance, Amussat relates an interesting case of occlusion of the vulva from close apposition of the posterior surface of the bladder to the rectum, in which he succeeded in forming an artificial vagina, by pushing a way, with his finger between the two viscera, and thus by degrees reaching the neck of the uterus. The case is detailed at length in the work before us, and deserves a careful perusal. In juxtaposition to this successful interference is a fatal one, related by de Haen, in which the bladder was opened into, in an attempt to reach the womb through an imperforate vagina. Now, there is considerable difference of opinion about the proper time for adopting surgical interference Nélaton, one of the most able surgeons of the present day, and who has devoted much time to the study of this subject, invariably delays as long as possible; and only proceeds to open these tumours when either the pains they cause are no longer tolerable, or when he fears that, by ulcerating an exit for themselves through the rectum, a still greater danger may be incurred by fecal matters entering the cyst.

Another question which presents itself for consideration, is the *proper time* for the surgical interference to be put into practice. Some authors recommend that a menstrual period should be the time selected; but Dr. Bernutz is of the opposite opinion, and believes that there is much less danger in perforating the hematocele, through the vagina, during a period of quiescence, viz. about 8 or 10 days after a menstrual period; and especially cautions the operator not to attempt to press on the abdomen, by way of aiding the evacuation of the fluid. He also advises that no large opening of either the hymen or other structure should be made with a bistoury, but that a small trocar should be selected in preference, and at the same time

to avoid a too rapid evacuation of the fluid. He also advises not to leave a canula in the opening, as has been recommended by some; for he thinks that the irritation caused by the presence of a foreign body would be more prejudicial than any additional puncture which might have to be made on the previous opening becoming healed.

Having exhausted the subject of menstrual retention and its consequences, the author now enters upon the subject of *Peri-uterine hematocele and its varieties.* We were under the impression that for a long time we had been considering this disease, but it seems we had misunderstood the writer. Let us hear him speak for himself:—

"In the preceding memoir I have especially insisted upon the severest accident which may arise, under certain circumstances, in consequence of the menstrual discharge being retained, viz., upon the passage into the peritoneal cavity of the blood previously contained in the genital organs, and which would give rise to a more or less violent peritonitis, and to the production of an intra-pelvic tumour, which, since my first publication upon this subject in 1848, has received the name of *retro-uterine hematocele.* The date of this publication, together with all the time and labour I have devoted to establish the relation between intra-peritoneal hemorrhage and disturbed catamenial function, would justify me in considering myself, rather than any of my contemporaries, the discoverer of this form of hematocele. But I do not lay claim to it, because, as I already declared in 1848, this discovery is really due to Ruysch. But I do claim for myself the merit of having been the first to bring forward this discovery, which, up to this time had remained as a simple indication, and to have established the basis from which so many recent works upon hematocele have sprung—and that, notwithstanding the studied silence of M. Nelaton, who, up to the present time, has avoided making the slightest mention of my memoir, although he has largely drawn from it, for his lectures on hematocele."

The great Parisian surgeon evidently finds no favour in the author's eye, and further on we again find him at issue with our friend Nelaton.

"I will not, like M. Nelaton and his pupils, gather into one group extra-uterine conceptions with congenital imperforations, menstrual retentions, all sorts of hemorrhages—rupture of aneurisms and tubo-ovarian varices, with hemorrhagic peritonitis, and thrombus, and to this incongruous collection give the name of disease,—but I shall rather endeavour to discriminate one from the other these different varieties of bloody

effusions, the pathology of which it is most important to recognise."

Having in a few words shown the physiological analogy between the testicles in man, and the ovaries in woman, the author insists that the name hematocele should be retained solely in those cases where the blood is poured into the peritoneal cavity; whereas the extra-peritoneal effusion, resembling rather bloody infiltrations of the scrotum in man, should never be distinguished by that name, no more so than when they occur in the areolar tissue of the scrotum. Having established the real significance of the name, he eliminates from the consideration of the present work all *extra-peritoneal* effusions of blood, as also the intra-peritoneal ones, which depend on abdominal injuries, rupture of aneurismal sacs, perforating ulcers, and accidents from extra-uterine conceptions, and reserves the name of hematocele for those collections of blood only, which are analogous to effusions of blood into the tunica vaginalis. Genuine peri-uterine hematocele the author divides into three classes,—such as arise from laceration of either the ovary or the Fallopian tube—such as depend on impeded excretion of the menses;—and, lastly, such as arise from a general hyperemic condition of all or some of the genital organs, and in which, although there may be no difficulty of excretion, yet there is at the same time an oozing of blood into the peritoneal cavity, through the open ends of the Fallopian tubes. Of the first class are three cases, which the author details at length, and in which death occurred in two instances. The cadaveric section of these two showed the origin of the hemorrhage to have been a varicose condition of the "*plexus pampiniforme.*" In all these cases the *immediate* cause of the hemorrhage was severe bodily exertion. How can a case of hematocele depending on varicose condition of the ovarian veins be diagnosed during life? Dr. Bernutz believes that women who have borne many children, and in a comparatively short period of time, are more subject to this form of hematocele; and that, if suffering from varices of the extremities, and, still more so if they have a varicose state of the veins in the vicinity of their genital organs, the physician may be guided to infer from these facts that the case is one depending on varix of the *plexus pampiniforme.* The treatment he recommends consists in cold applications to the abdomen, leeches to the neck of the uterus, and absolute rest; and, strange to say, he not only condemns "une diète trop severe," but on the contrary recommends a certain amount of nourishment, as an anemic condition might increase the hemorrhagic tendency. We said, strange to say,

because, in French medical practice, "starvation" holds such a prominent place, and the phantom "inflammation" is so constantly haunting their imagination, that one is agreeably surprised to see them leave their "diète absolue," their "sangsues," and their shocking "tisanes," and adopt at least a more rational and physiological empiricism. Should the patient recover, then she should carefully avoid for the future all excessive fatigue, and especially keep at rest at the approach and during the continuance of a menstrual period.

The author speaks of excessive venereal excitement as a cause of hematocele; and quotes two cases related by Tardieu, in which death supervened so closely upon an orgie, as to give rise to suspicion of poisoning, but at the post-mortem the cause of death was patent; an accumulation of blood was found in the peritoneal sac, without any opening or laceration whatever either in the ovaries or the tubes. Where did the blood come from, then? The author supposes that, as in cases of direct violence, effusions of serum and blood take place in the tunica vaginalis of man, so in woman the same thing may occur without the necessity of a previous solution of continuity in the corresponding parts. Another cause of hematocele is rupture of the ovary; and at observation 7, quoted from Drecq, an interesting account is given of a case in which death supervened in thirty hours from this cause, and in which the lacerated ovary was found of great size, and more like the spleen of an individual dead from scurvy.

Observation 9 contains the details of another interesting case, in which, after the suppression of the menses in consequence of a mental shock, severe peritonitis set in; death occurred in three days, which was at the examination of the body found to depend upon the laceration of one of the ovaries. We could extract many other cases of interest; but we must pass on to the third and last memoir of the volume before us, which treats of intra-pelvic hemorrhages depending on extra-uterine pregnancies.

"Having given, in the preceding memoirs," says the author, "a general account of intra-pelvic hemorrhage arising from the organs of generation while in a state of vacuity—we conceived that the subject would be incompletely dealt with, if we did not devote some space also to those intra-pelvic hemorrhages which occur during pregnancy, whether intra or extra-uterine. We shall, however, only occupy ourselves with the latter, not only because we consider the accidents depending upon the former to belong, strictly speaking, to the domain of obstetric science, but principally because these hemorrhages,

with very few exceptions, depend either upon apoplexy of the ovaries, or rupture of the Fallopian tubes or of the venous plexusses, and belong to the pathology of thrombus, already so ably treated by Deneux (Recherches pratiques sur les tumeurs sanguines de la vulve et du vagin, 1835), that we could add nothing to it; and our description would only be a paraphrase of his original work, to which we refer our readers."

Although extra-uterine conceptions are rare, yet they are sufficiently frequent to have enabled the author to collect forty-two cases, from the study of which he has deduced the following conclusions.

When hemorrhage occurs as a consequence of extra-uterine pregnancy, death almost always follows so quickly on the accident, that rarely is there sufficient time for the formation of an encysted hematocele.

The origin of the hemorrhage being various, he takes it as the basis of his classification, and divides all hematoceles, arising from this cause, into five orders :—

1. Caused by the rupture of the dilated utero-ovarian veins.
2. Caused by rupture of the ovary.
3. Caused by rupture of the Fallopian tube.
4. Caused by rupture of the fœtal cyst itself.
5. Caused by hemorrhage into the fœtal cyst, without any solution of continuity of its walls, and which, without any flow of blood into the peritoneal sac, may rapidly induce the death of the patient.

Now, considering that this portion of the work before us is devoted to the consideration of those sanguineous effusions, which depend *especially on extra-uterine pregnancies*, we were not prepared to find classed under the first, second, and third divisions, cases of hemorrhage, which might just as easily occur during an ordinary pregnancy, or without any pregnancy at all; we would, therefore, really be justified in skipping over all the author has to say upon the subject of his first three classes, and pass on at once to the review of the fourth and fifth; but having followed him step by step through his interesting, although prolix and wordy, publication, we shall follow him to the end, even through those cases which have no bearing on the subject in hand.

Observation first, taken from the writings of Ollivier D'Angers, is a case of laceration of the utero-ovarian veins, through which four pints of blood were poured into the sac of the peritoneum, causing death within seven hours from the occurrence of the first symptom. Just above the dilated plexus of veins

from which the hemorrhage had taken place, the Fallopian tube was dilated for the space of about three inches in one direction, and one inch and a half in the other; and upon this being opened, a fœtus of about five weeks was discovered; thus proving this case to have been one of extra-uterine conception. But we cannot at all see the necessity of making the varicose condition of the ovarian veins, and the subsequent rupture of them, to *depend* upon the abnormal pregnancy, as both might have existed independently of each other.

Case 2 is also one of extra-uterine conception, in which death occurred five days after the appearance of the first symptoms. The real nature of the case was diagnosed by M. Nonat during life. But at the cadaveric section was found what had never been anticipated, viz., pregnancy in the Fallopian tube *of one side;* whilst the origin of the hemorrhage was *ovarian apoplexy of the opposite side.* So this case also, equally with the preceding one, could have occurred quite independently of any conception at all.

Case 3, extracted from the "Mémoires de l'Académie des Sciences, 1702," is one of extra-uterine pregnancy, in which death occurred at about the sixth week, three days after the patient falling upon her knees. At the post-mortem examination, the left Fallopian tube was found torn, and in the rent, a vesicle containing a fœtus.

Case 4 is similar to the preceding one, with this peculiarity, that it would appear there was at the same time both extra-uterine and intra-uterine pregnancy.

Case 12 is also one of extra-uterine conception, with intra-pelvic hemorrhage, which last had been recognised during life. But the pregnancy not being suspected, and in the idea that it was a case of peri-uterine hematocele, a puncture was made into the tumour through the vagina; and after letting out some blood, but no clots, an injection of tepid water was thrown up through the incision, when immediately the patient complained of agonizing pain; she became deadly pale; rigors, followed by unceasing vomiting, set in; and in ten hours she was dead.

Space forbids us adding any more to this already lengthy analysis of a work which, even though tediously written, still deserves the attention of those who make diseases of females their special study; and to whom we recommend it, with the full conviction that they will feel interested, at the same time that they will gain information in reading the many carefully-related cases which form the bulk of the work.

On Diphtheria. By EDWARD HEADLAM GREENHOW, M. D., &c. London: Parker and Son. 1860. 8vo, pp. 274.

DR. GREENHOW seems to have been stimulated to write this excellent treatise, by his appointment under the Privy Council, in the year 1859, for the purpose of making an inquiry into the causes, symptoms, and treatment of diphtheria. The report of the results of that inquiry has already been published, at any rate *printed*, in the second report of the medical officer (Mr. Simon) of the Privy Council. In the treatise before us, the author incorporates his private experience and research with the products of his public labours, as a sanitary investigator, on the largest scale.

In the first chapter, we are presented with a "definition of the disease." In performing this task, the author falls into a description of its general phenomena, and does not attempt a strict definition. The truth is, that, supposing the author's view to be correct,—which presupposes the unity of epidemic sore throat without membranous exudation, and of true membranous angina attended with pellicular deposit,—the only basis of a completely satisfactory definition, such as has been put forth by Bretonneau and the French school, is at once cut from under us. We assent, in the main, to the author's doctrine, though it is most inconvenient in a nosological point of view.

It must not be forgotten, however, that we cannot pronounce rigorously a given throat to be a case of diphtheria, unless some part of the faucial or buccal mucous membrane displays a whitish, whitish-yellow, or buff-coloured membraniform layer, more or less extended, frequently stratiform, and for the most part easily removed, leaving the subjacent parts red, with or without minute red puncta, either perfectly whole, or partially broken up. The author's description of this exudation is true to nature, both in its positive and negative aspects; for we are all aware how much nonsense has been written upon this subject.

"Hemorrhage from the nose and throat, independently of the co-existence of purpura, often occurs in the course of diphtheria, and is sometimes very profuse."—p. 8. It is remarkable how constantly the earlier authors of the 16th, 17th, and 18th centuries allude to this symptom, and always as being one of very sad augury. We have seen fatal cases from hemorrhage during diphtheria, and strongly urged the physician to give a guarded prognosis, even when a moderate flow of blood comes from the

nares or fauces. It is agreeable to remember that the modern methods of treatment have disarmed this evil of half its dangers.

In the second chapter, diphtheria is described as it occurred in the 16th, 17th, and 18th centuries. The evidence, that the disease which ravaged various parts of Europe in these centuries—Spain in the 16th, Italy in the 17th, and our own country in the middle of the 18th century—is identical with that which has attracted the attention of the public in our times, is marshalled in lucid order, and produces the impression on the mind that further discussion on this point is superfluous. In fact, we may fairly challenge the most learned critic to state another disease, the description of which by various authors, far removed in place and time, presents so much similarity. In reference to Britain, the name of Fothergill, whose "account of the sore throat attended with ulcers", was published in 1748, deserves honourable mention. His essay is a masterpiece of learning and of acute observation, but has contributed much to the confusion of some authors, owing to his having described cases of scarlet fever with membranous sore throat, as examples of his gangrenous angina. The opposite error was made by the accomplished Withering, who confounded sore throat with scarlet fever, as shown by the very title of his book, "An Account of the Scarlet Fever and Sore Throat or Scarlet Fever Anginosa; particularly as it appeared at Birmingham, in 1778." For our own part, we must express our gratitude to Fothergill, for having given us a ray of light in the darkness which enshrouded the beginning of this epidemic. When, in 1856, we first saw cases, and uniformly fatal cases too, of this cruel malady, we sought for help among the authorities of the profession. The delineations of our class books were found uniformly untrue to nature; and the treatment advised,—based, for the most part, on purely speculative grounds, or on the fancied *identity* of the epidemic diphtheria with sporadic croup, or, worse than all, founded on second-rate French imitations—we bitterly discovered to be something worse than nugatory. In this strait, how dear to us became the life-like outline of Fothergill! How clear and strong the common sense of the old quaker of the Second George's time! It affected our mind like a new discovery; and we loudly exclaimed, in lecture and in private, against the retrogression of British medicine[a].

The third chapter is devoted to the description of "diph-

[a] It may be well to mention that Fothergill's epidemic is the same as that described by Huxham, at Plymouth, the latter occurring two or three years later.

theria in the 19th century;" and we need scarcely state that the illustrious name of Bretonneau takes the foremost place here. His clear account of the local characters of the disease, the learned criticism with which his works abound of the labours of his predecessors—above all, the excellent characteristic term (diphthérete) which he introduced into medical literature—give him an importance in the history of the disease, which time will not obliterate. His reputation has, indeed, unfairly overshadowed that of all previous writers. No candid reader can peruse the essays of many predecessors of Bretonneau without seeing that in acute clinical observation their superiority is manifest. His title to our respect is mainly founded on his thorough recognition of the *whitish layer being not a slough, or anything of the nature of a slough, but a bonâ fide layer of exudation on the surface of the affected parts.* He believed this membraniform layer characterised the disease, hence he called it diphthérite. He pursued this view throughout every possible line of injury.

"To him (as Dr. Greenhow observes), no inflammation unattended by exudation is diphthérite; and no inflammation attended by exudation is diphthérite, unless it be propagable by contagion; the virus being the membranous exudation which forms the distinctive pathological feature of this disease."

Further, the physician of Tours teaches that a true inoculation is the only mode of transmission of the disease. These doctrines are seductive in their simplicity, and form the French gospel of diphtheria, but we do not doubt that they are too absolutely laid down.

Bretonneau bestowed the greatest attention on the croup-phenomena of diptheria; but, in every essential particular, his observations had been preceded by those of Dr. Mackenzie and Dr. Brown, of Glasgow:—

"In a brief paper, published in 1825, Dr. Mackenzie says that the exudation of fibrin in croup 'very frequently commences on the surface of the tonsils, thence spreads along the arches of the palate, coats the posterior surface of the velum palate, sometimes surrounds and encloses the uvula; and at last, descending, covers the internal surface of the pharynx and œsophagus, the larynx and trachea.'"

In May, 1820, Dr. Brown read a paper on the disease to the Glasgow Medical Society.

In 1821, two cases of the disease proved fatal, under Dr. Mackenzie's care:—

"In both, the fetor of the breath, and the sloughy appearance of the effused lymph, were remarkable. In the first case, I was surprised, on dissection, to find the tonsils and uvula entire, and coated over only with an effusion; for I had laid my account to find a gangrenous loss of substance in these parts. He now announced to several of his medical brethren, that what had been considered as ulcers and sloughs in this disease, were nothing else than effused lymph, the progress of which over the velum and uvula and towards the alimentary and respiratory passages, he had distinctly observed"[a].

It is remarkable how little interest was excited in Great Britain by these accounts, until the outbreak of the great epidemic still figuring in our bills of mortality. Boulogne was the scene of a frightful mortality from diphtheria, from early in 1855 to 1857, though the boarding-house keepers of that town stoutly denied it in the English papers. It also prevailed in various parts of France, in 1856; and it is worthy of note, that this French epidemic differed from those described by Bretonneau in the very important particular, that the disorder more rarely extended to the larynx, causing croup symptoms. This peculiarity has existed throughout the English epidemic, which began in the summer of 1856, in the north and central-midland districts. Dr. Greenhow justly remarks, that dropping cases, isolated specimens of the disorder, had occurred in various parts of the country for years previously, but they had attracted no notice. Soon the disease spread over nearly the whole country, but by no means with the rapidity which is so terrible a feature of cholera and influenza. Here, too, we observe a point of similarity with the disease, as delineated by the older authors.

Dr. Greenhow makes some interesting observations on the existence of a "diphtheritic taint" in other maladies during the prevalence of the epidemic; and quotes several good illustrations of this feature of diphtheria.

In the fourth chapter, the author has brought together, from numerous sources, proofs of the existence of "sporadic and endemic diphtheria" during the last thirty years, and passes on in the following chapter to the discussion of the non-identity of "diphtheria and scarlet fever."

The author candidly admits that he was at first inclined to believe that diphtheria is but a modified form of scarlatina. He

[a] Quoted by Dr. Greenhow, from the Medico-Chirurgical Review, N. S., vol. vi. p. 290.

is now, however, satisfied that, "notwithstanding their frequent concurrence in the same place, and their occasional coincidence in the same individual, diphtheria and scarlet fever are distinct diseases," p. 105. This opinion we have ever strongly held, and can only assent to the fact of a closer affinity between diphtheria and scarlatina than between the former and any other specific febrile disorder. The existence of albumen in the urine in many cases of diphtheria, so well pointed out by Dr. Wade, of Birmingham, has been much relied upon in proof of the identity of these disorders; but it must not be forgotten that the presence of this principle in the urine is common to many of the pyrexial disorders, especially to the exanthemata; and moreover that, as contrasted with scarlatina, there are points of difference of a striking kind. In diphtheria the albumen appears early, often disappears in the course of a day or two, and is almost never complicated with anasarca. The opposite to this holds good in scarlatina. Dr. Greenhow insists strongly upon the fact of repetition of diphtheria attacks, as differentiating the malady from scarlatina, and refers to illustrative cases. Judging from our own experience, we should consider a second attack of diphtheria to be almost as rare as in the exanthemata. An unequivocal example of a new attack in the same individual has not yet come under our notice.

The following chapter alludes to human and brutal diseases coincident with diphtheria. Among the former are enumerated puerperal and typhoid fever, erysipelas, carbuncles, and boils. Among the latter are the "eruptive disease," or "the foot and tongue disease," which appeared among the cattle of this country about the year 1839; and the murrain, or "lung disease," which broke out a year or two later than the preceding. It is a noteworthy fact, that these same brutal disorders prevailed also just before the outbreak of diphtheria in the middle of the last century. The whole chapter is very interesting.

After adducing a series of facts and arguments, showing that putrid effluvia, locality, &c., exercise no recognisable influence on the generation of the disease, Dr. Greenhow discusses the "communicability of diphtheria," and supports his view of its contagious nature by the detail of cases. But it is merely stated that the disease is "in some way or other communicated" — Bretonneau's doctrine of inoculation being thought to be doubtful. The facts reported to us from France, from time to time, seem to prove that the disease may be communicated by this method. Not to allude to the well-known cases of Valleix, &c., another illustration has just occurred,

while we write, in the person of Dr. Gendron, who obtained his fatal attack, owing to a quantity of the secretions of the mouth being suddenly expectorated on his face during the performance of tracheotomy. We strongly concur with Bretonneau's doctrine, so far as relates to inoculation being *one* of the modes of transmitting the disease; but we cannot believe this to be the only mode of conveying it. We would here draw attention to the fact, that diphtheria is far less contagious than scarlatina or variola; and we believe its *contagious sphere* to be more circumscribed than that of these maladies. In a large house, children will be often safe if they are completely secluded from the sick chamber.

The following chapters are occupied with a careful and complete account of the symptoms, grades, and varieties of the disease, and of its morbid anatomy:—

"Low forms," says the author, "of cryptogamic plants are occasionally found on the exudation, a circumstance which gave rise to the belief, that the disease is of parasitic origin. This opinion is disproved by the facts that, on the one hand, the supposed parasite is not invariably present in diphtheria; and, on the other, that it is frequently found on unhealthy mucous surfaces, which are not of a diphtheritic nature. Examined under the microscope, the exudation is found to consist of coagulated fibrine and epithelium; the latter being usually more abundant in the outer portion or layer of membrane, whilst the deeper portion is more purely fibrinous. But, in this respect, there are numerous variations. Exudation-cells are often intermixed with the fibrillated texture."

The last chapter is devoted to the treatment. Dr. Greenhow is adverse to the rude local treatment, now so much in vogue, and condemns the stronger applications, excepting in the earlier stages of the disease. We are disposed to agree with much that he offers on this point, being well assured that these measures have been used too freely, and, above all, *for too long a period.* But we would not willingly dispense with the *moderate and early* employment of the dilute hydrochloric acid, or of the same acid a little weaker than the dilute acid of the London Pharmacopœia. The employment of strong solution of nitrate of silver should be limited to cases where either the exudation has not yet appeared, or has been peeled off mechanically, or dissolved by the acid application.

As regards the general management, we can earnestly recommend the perusal of the author's cases, as well as of his remarks, which, in the main, agree with our own experience. The remedy which he has chiefly relied upon, judging from the detail of the cases in the eighth and ninth chapters, is the

tincture of the sesqui-chloride of iron; for, wherever the treatment is alluded to, this remedy is included in the prescriptions. We congratulate the author on the marked success of his measures, which seem to have greatly limited his opportunities of *post-mortem* inquiry, so far as concerns his own clientele—one case only, where the steel treatment is said to have been used, is stated to have died. It was a young girl, aged 14, and it is obvious that her life was sacrificed by her own mad obstinacy. The author's *doctrine* upon this point is as follows:—

"The tincture of sesqui-chloride of iron has been so generally adopted by the profession, as to afford a guarantee that it has in the main been useful in their hands. There are, nevertheless, cases in which I have found chlorate of potash more useful; and there are others in which the combination of the latter with the tincture of iron is a better remedy than either of them separately."

Our *practice* agrees with that of Dr. Greenhow, as shown in his cases. Our *doctrine* upon the steel treatment is in its favour, without any reserve whatever.

This treatise is a valuable addition to British medical literature. It will ever be a necessary book of reference on the history of diphtheria; and, in a practical aspect deserves the perusal of all who are likely to be called upon to cope with the formidable disorder of which it treats.

The Natural History Review. A Quarterly Journal of Biological Science. London: Williams and Norgate. No. I. January, 1861.

This is a fresh issue, and a developed one, of the "Irish Natural History Review, and Quarterly Journal of Science;" and, as the Editors announce, is intended to be "A Quarterly Critical Journal of Biological Science, which, without interfering with existing scientific periodicals, shall stand in the same relation to naturalists and other persons interested in Biological inquiries, as that which is occupied by the ordinary quarterly reviews in respect of men of letters and the general public." This is a large promise, and one scarcely likely to be fulfilled, judging by the first number. The principal paper in this Number is one "On the Zoological Relations of Man with [*sic*], the Lower Animals," by Professor Huxley, F.R.S. This paper is well worth a careful perusal, notwithstanding the manifest bitterness of its author against his antagonist (Professor Owen), and is occupied with the discussion

of the classificatory position of Man among the Quadrumana, on evidence furnished by the anatomy of the brain.

Linnæus ranked Man in the same order as the Ape, the Lemur, and Bat, founding his classification on an organ very different from the brain. Professor Owen, and several of the moderns, raise Man into the dignity of a Sub-class (*Archencephala*). Professor Huxley objects, as we think justly, to this arrangement; and joins issue with Owen on the three following points:—

1. The posterior lobe of the cerebrum, overlapping the cerebellum.
2. The posterior cornu of the lateral ventricle.
3. The *Hippocampus minor.*

The possession of these three peculiarities, according to Owen, is the prerogative of the brain of Man. Huxley denies that any of them is peculiar to Man.

The evidence against Owen's view, on the first point, is overwhelming, including the authority of Tiedemann, Cuvier, and what Professor Huxley maliciously calls "an authority which, I am sure, Professor Owen will regard as irrefragable," viz., Owen himself. He also quotes Macartney, Sandifort, Vrolik, Geoffroy St. Hilaire, Schrœder van der Kolk, Gratiolet, and his friend, Dr. Allen Thompson. He might also have added a higher authority than any of those named, viz., the monkey's brain itself, which, in all the species we have examined, overlaps the cerebellum fully. Professor Huxley spends much of his force on this first, and least doubtful, point, but is much less satisfactory in his attack on the *posterior cornu* and *Hippocampus minor;* his principal argument on the *posterior cornu* being a sneer at Owen for having borrowed a plate from another anatomist without acknowledgment; while with respect to the *Hippocampus minor,* his chief authority is the work of the Brothers Wenzel, who say, with respect to the "*Tuber in cornu posteriore ventriculorum lateralium :*—Non semper plerumque tamen adest, et quidem utroque in latere sive in utroque cornu. Inter quinquaginta et unum, eo specialiter fine a nobis examinata cerebra diversæ omnino ætatis atque utriusque sexus, tria tantum reperiebamus in quibus tuber illud in utroque latere, et duo in quibus uno in latere desiderabatur."

We have, ourselves, met with human brains, in which it was, to say the least, exceedingly difficult to demonstrate the existence of this hippocampic tuber.

On the whole, we believe Mr. Huxley has the facts on his side of this controversy, and we regret, therefore, that he has

shown so much personal feeling in the matter. Having pushed man from his *archencephalic* position, and reduced him to his proper level among the *gyrencephala*, he seems to think that he has made out a case for what he pedantically calls "the pithecoid pedigree" of man. We decline to follow him into this region of poetry and speculation, which we do not believe to lie within the domain of science, and which is only conversant with what he calls "orders of facts, which are neither demonstrable nor discernible," and which are as remote from science and logic as the metaphysical theories at which he pretends to sneer.

The other papers of this number of the new Review do not fall within our provice; but, on the whole, they appear to be rather heavy reading, not as attractive as the Quarterly or Edinburgh. Dr. M'Donnell's paper, "On the Electrical Organ of the Skate," is an old friend, the substance of it having been read some time ago before the Royal Irish Academy. In a paper on the *Axolotl*, the author's Latin strikes us as being somewhat peculiar, the word *ovæ* occurring repeatedly, in the sense of *eggs;* and the word *amphibia* being used as the singular nominative, agreeing, no doubt, with *animal*, understood: the author of this paper is a Master of Arts, of Dublin and Oxford; surely he ought to have known better; at least, as a naturalist, he should have been familiar with the Latin for eggs, from the maxim—

Omne vivum ex ovo.

The following passage would have delighted Cobbett as an example in English parsing:—

"Surely such embryological distinctions point to, at least, a difference in the general—in the ordinary acceptation of this word—of these creatures."

The editors of the Review are supposed to hold somewhat advanced views on natural science, and to follow Darwin and Huxley, *haud passibus æquis;* but it surely is not necessary to carry their ideas of progress so far as to invade our traditional notions on the subject of Latin declensions and English grammar. We hope we have seen the last of this slipshod writing.

On Organic Polarity; showing a Connexion to exist between Organic Forces and ordinary Polar Forces. By H. O. F. Baxter, Esq., M. R. C. S. E., &c. London: Churchill. 12mo. 1860.

In this little work the author deals with some of the most difficult questions of rational physiology; and we gather from the note to page 56, that the Royal Society refused to publish his papers "on the development of current force during lacteal absorption, nutrition in the muscular and the nervous tissues, and in plants," read before that Society, in 1852. This book appears to be an abstract of those papers, and we believe it will repay a careful perusal; although we do not pledge ourselves to its results, as we think the questions it deals with are not yet removed beyond the pale of reasonable difference of opinion.

The following preliminary rules will be found useful to those about to read the book for themselves, as there is at least some obscurity in Mr. Baxter's statement of first principles, and his book requires some previous knowledge of the subject of Voltaic Electricity.

1. When oxygen, or an acid, acts on a metal, the latter assumes *negative* tension, while the oxygen, or acid, assumes *positive* tension.

2. When a liquid acid acts upon a liquid base, e. g. sulphuric acid on soda, the base, like a metal assumes *negative* tension, and the acid assumes *positive* tension.

3. If an electrolyte be placed in the circuit, and *decomposed*, its particles are polarised oppositely to those of the *combining* fluid; its oxygen, or acid, appearing at the *positive* pole, and its hydrogen, or base, appearing at the *negative* pole of the circuit.

According to these statements the metal acted on chemically, the basis, &c., are *negative*, while the metal not acted on, the acids, oxygen, &c., are *positive*. Now, this is a matter of fact not depending on any hypothesis. How then comes it that language the very reverse is commonly used, i. e. oxygen, the acids, &c., are called *electro negatives*, while the metals acted on, such as zinc, hydrogen, the bases, are called *electro positives*? This mode of speaking is the offspring of a theory of Grotthuss, who supposed that the poles or extremities of the terminal wires of a battery, one of which was in a state of positive tension, the other of negative tension, exercised attractive and repulsive actions on the constituents of chemical compounds, such as water, sulphate of soda, &c., the constituents themselves

being supposed to be endowed with the electric tensions of the two kinds. According to this hypothesis, accordingly, all the bodies, such as oxygen and the acids which are developed at the positive wire, are *electro negatives;* the others, such as hydrogen and the basis which are developed at the negative wire, electro positives. Mr. Baxter applies these or similar principles to the explanation of the following facts:—

(*a*). When the electrodes of a galvanometer are brought into contact, one with the mucous surface of the intestine in a living or recently-killed animal, and the other with the venous blood from the same part, an effect occurs upon the needle, which shows that the electrode in contact with the blood is *positive* to that in contact with the mucous surface.

(*b*). Under similar circumstances, the electrode in contact with the gall bladder is *negative* to that inserted into the hepatic vein, or vena cava ascendens.

(*c*). The electrode inserted into the renal vein is *positive* with respect to that inserted into the pelvis of the kidney.

(*d*). The electrode inserted into a lactiferous vessel, and that inserted into a vein from the same part, are in opposite tensions, the latter being positive.

Mr. Baxter insists, that in these and similar cases of secretion, the phenomena are best explained by electrolysis:—

> "The arterial blood [or, blood supplied to a secreting organ] being, as it were, separated into its two elements, the *secreted product* and *venous blood* [or, blood leaving the organ] just as muriate of soda is decomposed and separated into its two elements, muriatic acid and soda."

We believe this conclusion of Mr. Baxter to be larger than his premises, inasmuch as no chemical decomposition can take place in a voltaic circuit, without a corresponding, and *more powerful* composition also taking place in some other part of the same circuit.

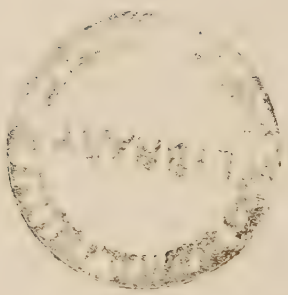
The fact of the opposite tensions of these secretions, and of the blood flowing from them, is important, but it admits of many and diverse interpretations. The same remark will apply to Mr. Baxter's explanation of other facts, such as—

(*e*). That, during nutrition in the muscular tissue, the muscular fibre and the venous blood are in opposite electrical states.

(*f*). That the blood of the internal jugular is positive with respect to the brain.

(*g*). That the external and internal portions of the muscles

and nerves are in opposite polar conditions. While we admit the full value of the investigations of Mr. Baxter and others on the electrical condition of the various parts of the body, we must be allowed to put in a plea for a simpler view of the matter; we believe all these electrical, calorific, motor, sentient, and even intellectual phenomena, are the results of the chemical changes the food undergoes before it is finally excreted; that such changes are accompanied by electrical, calorific, and motor effects, is now fully proved; and we believe more will be accomplished by physiologists in attending to the chemical phenomena of the organs of the body, than by directing their investigations to the electrical effects of those chemical phenomena which are, in reality, one step farther removed from the mystery of life, than the chemical actions are which produce them.



PART III.

MEDICAL MISCELLANY.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY[a].

(*Continued from* vol. xxix., p. 488.)

SESSION 1860–61.

OCTOBER 10, 1860.

DR. TANNER, PRESIDENT, in the Chair.

Cancer of the Pylorus; jaundice; anemia.—DR. FINN exhibited morbid specimens, exemplifying the above.

Ellen Haly, domestic servant, aged 35, unmarried, was admitted into the North Infirmary on the 30th April, 1860, complaining of irritability of the stomach, and palpitation of the heart. These symptoms dated from a year previous to her admission. Her general health had been good to that period, with the exception of the occasional occurrence of rheumatic pains, referred to the shoulders and lower extremities. Her mother had suffered from bad stomach, and died in consequence; she stated that the catamenia had disappeared about five months since, and that she has latterly been much troubled with costiveness; pulse, 90, feeble; tongue pale; and face anemic, with an expression suggestive of extreme anxiety. On examining the abdomen, a tumour of an oval shape, but presenting much irregularity of surface, was observed to occupy portions of the right hypochondriac and umbilical regions; when first observed, it was about the size of a small orange, but its volume subsequently increased. Manual examination, with a view to ascertain its outline, was attended with great pain; when pressed, it communicated a moderate impulse to the hand; and auscultation revealed a distinct arterial murmur, which varied in its character under the influence of pressure. A loud murmur was also heard

[a] These Reports have been furnished to us by Dr. S. Henry Hobart, Secretary to the Society.—ED.

over the præcordial region, and over the chest generally; and the veins of the neck exemplified the auscultatory phenomena usually associated with spanemia. The treatment consisted of the frequent employment of enemata, necessitated by the costiveness (already noticed), and of the internal administration of the nitrate of bismuth, which latter medicine appeared to exert very considerable influence in controlling the irritability of stomach.

On the 4th of September, the surface became generally jaundiced, the jaundice having been ushered in by an aggravation, for some days previously, of the gastric symptoms.

5th. Irritability of stomach has increased much since yesterday, a large quantity of dark, grumous matter, having been frequently rejected during the night.

From this period to that of her death, on the 28th September, she suffered from acute pain, referred to the abdomen generally, and to the vicinity of the tumour more particularly, which pain increased in severity towards evening and night.

Autopsy.— On exposing the abdominal viscera, the tumour above noticed was observed to be situated near the free edge of the right lobe of the liver. The gall-bladder, a portion of the transverse colon, the head of the pancreas, and the pylorus, severally contributed to its production, having mutually contracted rigid adhesions at that point.

The point of departure of morbid change, however, appeared to be the pylorus, which was the seat of cancerous degeneration, the latter involving also the commencement of the duodenum. A circular portion of the pylorus, to the extent of a shilling, had been destroyed by ulceration; and its circumference, at every point, was united by adhesive inflammation to the fundus of the gall-bladder. The latter had thus, in a remedial capacity, combined the office of a mucous membrane with that of maintaining the integrity of the intestinal canal. The ductus choledochus was much enlarged; the mucous membrane of the stomach was pale, with the exception of the part immediately adjoining the pylorus. Towards the greater curvature were observed some air-bubbles, about the size of a pea, in the sub-mucous areolar tissue. The liver was congested, but not altered in its form, though somewhat diminished in volume; it offered an example of cirrhosis in its incipient form. The pericardium contained a small quantity of serous effusion. The heart was pale and small; at a short distance above the aortic valves, some atheromatous deposit was observed.

Case of Pulmonary Apoplexy, with fibrous clots in left ventricle, and partial Pericarditis.—By Dr. Cummins.

The following most painful case seems to me so interesting, that I shall not apologise for entering into its minutest details:—

Mr. H., aged 63, a short thickset man, of active, temperate habits, who had always enjoyed excellent health until the winter of 1859–60, caught cold during the very severe weather, and suffered

from cough for some time, without seeking medical advice. I was called to him on the 23rd of February, and informed that he had been much worse the last three weeks; complaining of palpitation, bad cough, and swelling of feet and legs, but no difficulty of breathing. He still persisted in refusing to call in a medical man until the evening I first saw him, when urgent dyspnœa suddenly set in, and obliged him to seek relief. His wife informs me also that for some time past his sleep has been much disturbed by frightful dreams. My first visit was paid at 9 p.m., about half an hour after the commencement of dyspnœa. I found him sitting up in the bed, gasping for breath, with an occasional dry cough at the end of expiration. Pulse regular, feeble, and so rapid that I was unable to count it. Heart's sounds quite inaudible, and no impulse to be felt; cardiac dulness normal; respiratory murmur heard loudly, without any prolongation of expiration, over every part of both lungs, mixed here and there with some dry and moist bronchial ronchi, but no dulness on percussion; respirations 48 in the minute. I administered ether, sal volatile, and chloroform, one after the other had failed, without affording any relief. I ordered an enema of turpentine, to remove some flatus which was present. Large mustard plasters were applied over chest, neck, and epigastrium. A strong emetic was given, and failed to make him vomit, even when repeated. He also got five grains of calomel; and a large blister was applied to the cardiac region. During all this time, I repeatedly examined the heart, without being able to detect the least sound. It was fearful to witness the agony of the orthopnœa without being able to relieve it, and to stand by, watching the failure of one remedy after another, and only able to obviate the dreadful exhaustion and sinking by large doses of stimulants.

Towards morning the lips were quite blue, the face pallid, and covered with cold perspiration, the pulse at the wrist scarcely to be felt; and still the dreadful dyspnœa continued, preventing the exhausted patient from lying down, or snatching a moment's repose. I now prescribed the ethereal tincture of lobelia, in ten-drop doses every hour, with stimulants; and before I left him there was a slight improvement. Shortly after being relieved, he was able, though still greatly distressed, to lie down.

Dr. Tanner visited him with me in the afternoon, about five o'clock, when we found that the dyspnœa had almost left him; that he had dozed a little during the day; and that the lips, although still blue, were improved. Heart's sounds still quite inaudible; pulse at wrist scarcely to be felt; but over entire cardiac region is now to be heard synchronous with respiration; a metallic tinkling, which can be traced downwards to the umbilicus, and is evidently propagated from the stomach. The bronchial rales have rather extended, but there is no increased area of cardiac dulness or other abnormal sign in any part of the chest. There has been some transient delirium during the day. We directed that the lobelia

should be continued, and ordered a mixture of ether and sal volatile every two or three hours.

At half-past seven o'clock I visited him again, and found him greatly improved in appearance; the heart's sounds quite distinct, but accompanied towards the base by a most marked friction. I immediately ordered three grains of calomel, with a minute quantity of opium, every third hour; and applied another blister over the heart, as the first had not risen well.

February 25.—Spent a good night, and seems better; friction not so intense, but no increase of percussion sound; an obscure endocardial murmur is heard toward apex. Ordered mercurial ointment to thigh, blistered surface, and armpits; half a grain of calomel every second hour. 2 P.M. Dyspnœa has again returned, but not so urgently as before. Bowels have been opened. 8 P.M. Continues much in the same state; friction gradually diminishing.

26th.—Was rather delirious during the night. Heart's action more extended, and friction indistinct; no marked extension of dulness. Pleuritic friction sound heard in some spots over anterior part of left lung. Bronchial rales rather increased, especially over right lung, where the vesicular murmur is also observed. Dyspnœa much less urgent; some distention of jugular. Urine scanty, and high-coloured; bowels open; pulse still almost imperceptible, notwithstanding the increased cardiac action. Continue powder as before; dry cupping over both lungs; omit lobelia. Ordered a mixture of ammonia, squills, senega, &c., every third hour. 4 P.M. Pulse distinct, and rather hard; heart's action violent; dyspnœa very urgent. Omit the mixture, and take the lobelia as before; continue powder; to have a turpentine enema. 10 P.M. Dyspnœa less urgent.

27th.—Slept little during the night; very restless; expectorated during the night much sero-sanguineous fluid, which still continues, and has relieved the breathing. Jugular distended; friction, if present, very indistinct; heart's action tumultuous, and extended. Ordered 3 grains of calomel, 1 grain ipecacuanha powder, 1 grain powdered squills; one powder to be taken every third hour. 10 P.M. The expectoration has continued all day; pulse very rapid, but distinct; breathing better; countenance and lips very livid.

28th.—Sinking rapidly. Respiration failing; pulse very rapid, but distinct; mind wandering. Two hours before death he was gasping convulsively for breath, and died at 8 P.M., after a hard struggle. The mercury had no effect on the mouth.

Post-mortem—Twenty-four hours after death.—Muscles well developed; much adipose tissue; rigor mortis ordinary; lips perfectly livid. On opening pericardium, we found a considerable quantity of serous effusion, and a recent lymphy exudation covering the anterior part of right ventricle; the remainder of heart and parietal pericardium being free from it, no adhesion of any part. Heart somewhat enlarged, but firm, and its muscular fibre well developed; valves and endocardium generally healthy; left ventricle

full of fibrinous clots; right side of heart distended with black blood. There is some effusion into both pleural sacs, but no adhesion. Base of both lungs a complete non-crepitant mass of blood; remaining portion of both organs intensely congested, and infiltrated with blood and serum in many parts. Bronchi much congested, and filled with a sero-sanguineous secretion, such as had been expectorated. Liver somewhat enlarged. We were obliged to perform the sectio cadaveris very rapidly; and, having promised not to soil the bed-clothes, were prevented from examining the parts as minutely as we should have wished.

From the first moment I saw this patient, I made up my mind that I had to deal with no ordinary case of spasmodic asthma; and still, at first sight, it appeared as if the orthopnœa were caused by some lesion of the pneumogastric nerve—a belief which might have derived support from the impossibility of acting on the stomach by emetics. It occurred to my mind that a parallel might be drawn between it and the dyspnœa, disproportionate to the amount of bronchitis present, observed in influenza, and attributed by some pathologists to a diminution of the "vital activity" which the lungs derive from the pneumogastrics; for here the dyspnœa was certainly out of all proportion to the slight bronchitis. But section of the pneumogastric nerves, instead of causing rapid respiration, has been found to diminish the number of respirations considerably; and here they amounted to forty-eight in the minute; so it was at once evident that there was at least no diminution in the nervous supply. What, then, was the cause? My attention was early directed towards the heart for information. Its sounds were inaudible, and no impulse was to be felt; still the character of the dyspnœa was very different from the "respiratory distress" of fatty degeneration of the heart; while the history and symptoms of the case also tended against that view. The conclusion I at last arrived at was, that there was some obstacle to the circulation through the left side of the heart, with passive congestion and serous infiltration of the lungs; and this opinion was formed long before sero-sanguineous expectoration confirmed it. Notwithstanding the amount of stimulants taken, reaction was long in being established; and when, at last, it came, with it came a new complication of the case, viz., partial pericarditis. It is a question fairly open to discussion whether the symptoms when I first saw him were not due to the first stage of pericarditis, obscured as to its physical signs by the absence of the cardiac sounds during a period of collapse. My own impression is, that the first link in the chain of morbid action was the formation of the fibrinous clots in the left ventricle; the second, congestion of the lungs; third, pericarditis. And the suddenness of invasion by the dyspnœa is in favour of this view; as is also the fact that there was complete absence of the physical signs of pericardial effusion in the commencement.

October 24, 1860.

Dr. Tanner, President, in the Chair.

Case of Cancer of the Gall Bladder, and neighbouring tissues. By T. W. Belcher, M. A., and M. B., Oxon. and Dub., L. K. Q. C. P. I. —Denis Donovan, the subject of the following remarks, died in the hospital of the Cork workhouse, Oct. 13, 1860. During a lingering and painful illness he had been under the care of Dr. Townsend, at whose request the body was examined after death:—

The patient was a man of about fifty years of age; had been a hard drinker in earlier life; and when under medical treatment, exhibited several symptoms of internal cancerous disease. The general emaciation was extreme, while the abdomen continued tumid, and in some places had a nodulated feel. The countenance gave expression to an appearance of intense anxiety. The two prominent symptoms were, constipation of the bowels, and ever-recurring sickness of stomach.

The treatment consisted in medicines, administered with a view to relieve both of these conditions, and nourishing diet. Purgative medicines, given by the mouth, were found to be useless, so that enemata were in perpetual use to overcome the first of the symptoms already referred to; while drinks of milk and lime-water, blisters over the region of the stomach; acetate of morphia, and watery extract of opium; and pills composed of trisnitrate of bismuth, with sulphate of quinia, were exhibited for a considerable time. Under the last-mentioned combination he rallied a little, but finally sank and died, as before mentioned.

The post-mortem examination was undertaken the following morning; when, beside the great emaciation already referred to, very considerable depression existed above and below both clavicles; the abdomen was enormously distended, and though only twenty-four hours after death, was quite green in colour. Great care was taken in dissecting the skin from that region; but so thin were the abdominal parietes, that the first touch of the knife penetrated the colon, when a large quantity of feculent matter, of the most offensive odour, shot up with much force, and continued to exude so long as the examination lasted. On opening the thorax, a most horrible discharge of fetid gas took place, so that all concerned had to leave the room instantly.

The lungs were dark and congested; the heart natural, and the thoracic viscera, in other respects, healthy. The liver was found of the natural size, and apparently healthy. The gall-bladder was replaced by a cancerous tumour—the entire contents of the lesser omentum being one cancerous mass. The posterior surface of the mass was nodulated, and contained gall-stones in numbers. The ascending colon was large, so large as to resemble the stomach in general appearance; the junction of its ascending with its transverse portion was involved in the disease, and so strictured that the finger

could be passed through it with difficulty. The right kidney was enlarged; its pelvis cancerous, and the whole involved in the diseased mass. The left kidney of its normal size, and in its usual position.

The diseased condition of the vital organs just enumerated must have been of long standing, and affords a striking instance of how long the human constitution can withstand the inroads of a mortal foe. The stricture in the great intestine evidently caused its distension, as, beyond the seat of the disease, it was of its natural size, and empty; in like manner with the stomach; thus accounting satisfactorily for the two main symptoms of the case—the constipation of the bowels, and constant sickness of the stomach. How the functions of the gall-bladder were performed, if at all, remains a question for the medical critic.

Dr. Belcher also read the following case, and exhibited the pathological specimens taken from the subject:—William Casey, aged 60, married, a pensioner, who served some years in India, and was formerly of intemperate habits, was admitted into the hospital of the Cork workhouse under the care of Dr. Wm. C. Townsend, Oct. 6, 1860.

He complained of debility, and troublesome cough; felt palpitation at the heart for a month previous to admission; never had rheumatism; height, 5 ft. 10 in., and wasted in general appearance; is troubled with dyspnœa, and has not any desire for food; his tongue is clean; legs swollen; bowels regular; kidneys act; but does not sleep; changes his decubitus from one side to the other with difficulty and uneasiness. Circulation feeble, irregular, and intermitting—almost imperceptible at left side. Chest everywhere clear on percussion; bronchial râles all over the lungs, with mucous expectoration more or less tinged with blood. The heart's action feeble, very rapid, with occasional regurgitation, but without any trace of murmur; no increase of præcordial dulness; jugular pulsations very evident at both sides. Ordered, tincture of opium, 12 drops; compound spirit of ammonia, half a drachm; water, 1 oz., for a draught; 6 oz. of wine, and beef-tea.

12th. Lies entirely on right side; pulse almost imperceptible in either wrist; biliary tinge all over the body; tongue clean and cold; urine scanty, and highly loaded with bile; heart's action feeble, and extremely rapid; swelling of left leg very much increased; extremities cold; complains of want of sleep. Repeat the nourishment.

13th. Presents the collapsed state of a cholera patient; no pulse to be felt at wrist; decubitus now on the back. Repeat 6 oz. of wine, beef-tea. Died at 2 P. M., being the eighth day after admission.

Post-mortem examination.—Oct. 15th. The chest was externally in good condition; and although the muscles were somewhat wasted, yet fat was found everywhere on them. On opening the thorax, both lungs—but especially the right—presented a highly congested appearance. The right pleura contained a large quantity of fluid, while the left lung strongly adhered to the walls of the chest. Its

apex was so firmly attached, that considerable force had to be employed in dislodging it from its position, and this was only effected by leaving parts of the pulmonary substance behind. The right lung, when cut into, appeared engorged, and its superior portion carnified to a great extent. The heart was firmly adherent to the pericardium, which latter was glued to the left lung. The pericardium was greatly thickened, and loaded with fat throughout its entire extent. The heart's chambers were dilated, but its muscular substance not enlarged; the latter had the fawn-coloured tint usually understood to belong to fatty degeneration of that organ. The valves, with the exception of some fatty deposits, were, in other respects, in good working order. The left lung was loaded with miliary tubercles, except at its lower portion, where about one-fourth of its tissue was apparently healthy. The apex of the right lung was also studded with tubercular deposit, and had a cicatrix which did not appear to have been adherent to the pleura. The middle lobe presented a pneumonic appearance, while the lowest portion was tubercular.

Remarks.—In this case the congested state of the right lung seemed due to the constant decubitus on that side, while the system was greatly debilitated. The heart, on close comparison with another of healthy size, was not really much enlarged, while the presence of fat in its tissue was quite evident; nevertheless, the ventricles were greatly dilated. The firm adherence of the pericardium, as well to the heart as to the lungs, would seem to have resulted from a former diseased condition of the system; while the abnormal state of the parts examined defied any clear traces of the coronary arteries. The kidneys should have been examined, but time was only afforded to open the thorax; after which the body was removed by friends of the deceased.

November 16, 1860.

Dr. O'Connor, Ex-President, in the Chair.

Case of Pneumonia and Fetid Abscess of the Lung.—Dr. W. P. Bernard exhibited the lungs of a patient who had died in the workhouse hospital, and related the following particulars of the case:—

Matthew M'Carthy, aged 68, labourer, of intemperate habits, was admitted into workhouse hospital, under the care of Dr. W. C. Townsend, on 19th June, 1860, suffering from great debility and paroxysms of coughing, coming on with extreme severity at irregular intervals.

He states he enjoyed excellent health until the year 1831, when he suffered from influenza. From this time be has never been many days free from harassing cough, which has become much more constant and distressing within the past few months.

October 15th. Nervous temperament; countenance anxious, and livid; pulse 100, feeble, and irregular; respiration 40; tongue clean

and moist; bowels act fairly; urine scanty, and high-coloured; no desire for food; chest clear on percussion, anteriorly and posteriorly; bronchial râles scattered over anterior portion of both lungs; decubitus entirely on the back; is free from pain, but suffers from frequent cough and dyspnœa, coming on in paroxysms of alarming severity, leaving the patient bathed in perspiration, and completely exhausted.

23rd. Is suffering from pneumonia, having exposed himself to cold whilst heated after one of his fits of coughing; complains of pain in left side; circulation and respiration quickened; much fever present; entire of right lung clear on percussion; left lung anteriorly clear above, but marked dulness over root and base, with absence of respiration posteriorly; distinct crepitation heard; expectoration profuse and bronchitic. To have five grains of Dover's powder every four hours, and four ounces of wine, with beef-tea, throughout the day.

24th. Feels weaker; expectoration tinged with blood; bronchial breathing over both lungs; a blister to be applied over root and base of left lung posteriorly; powders to be continued, and wine increased to six ounces; no chlorides in the urine.

25th. Spent a quiet night; feels better.

26th. Pulse 100, very small, irregular, and feeble; expectoration less profuse, tenacious, with rusty tinge; tongue slightly coated; urine loaded with bile and lithates; no chlorides present. To have half an ounce of oil; wine and Dover's powder to be continued.

27th. Slept fairly last night; looks and feels better; pulse 96, feeble, and regular; sputa bronchitic, and free from rusty tinge; pneumonic symptoms have disappeared, leaving him in nearly the same state as before the attack of pneumonia; to have (at his own request), instead of the wine, one ounce of whiskey made into punch, three times in the day. Dover's powder not to be given more than three times daily.

28th. Pneumonic symptoms have reappeared; prune-juice expectoration; increase of dulness over left lung posteriorly; anteriorly, distinct crepitation above left nipple, extending upwards, midway to clavicle. Dover's powder to be continued every four hours, as at first.

29th. Cough still very troublesome; circulation 100; respiration 32; skin cooler; expectoration not so characteristic as on last report, but fetor from it and the breath noticed for the first time. Dover's powder to be continued, and wine six ounces, substituted for whiskey; refuses his beef-tea.

November 1st. Less cough; pulse 96; respiration 23; fetor from breath and sputa very marked; base of left lung much clearer posteriorly on percussion; distinct crepitation on inspiration and expiration. Continue Dover's powder and wine, and, in addition, a table-spoonful frequently of a mixture, containing—syrup of tolu, 3 ounces; Hoffman's anodyne and laurel water, of each 3 drachms; mucilage, two ounces, and water to make up an 8-ounce mixture.

November 3rd. Slept better; pulse 100; respiration 48; cough

still very distressing; sputa rusty, and very fetid. Treatment to be continued, substituting 3 ounces of wine for the spirit.

5th. Extreme prostration; pulse 116, regular, but very feeble; respiration 34; tongue dry and red; expectoration less tinged, but fetid. Continue wine and Dover's powder.

7th. Still weaker than last report; cough nearly disappeared; expectoration scanty, resembling very fetid pus, with patches of a sanious character floating on the surface.

9th. Died at 8 A. M.; having had no cough for many hours previously.

Autopsy—Thirty hours after death. Body greatly emaciated; muscles of the thorax healthy, with some adipose tissue; immediately on raising the sternum, surface of lungs appeared perfectly healthy, but quickly and perceptibly collapsed, particularly *at right side;* left lung free from adhesions anteriorly and superiorly; posteriorly and inferiorly adherent to the pleura costalis, and to the pericardium (the latter being much thinned, and almost transparent); right lung slightly adherent superiorly, more firmly posteriorly, and to the diaphragm. From the extreme base of left lung a quantity of fetid pus escaped from an abscess—the sac of which would have contained a large-sized orange; posterior portion, inferior portion, and base of right lung were hepatized; bronchial glands very much enlarged.

Poisoning by the Fumes of Charcoal. By JOHN POPHAM, M. D., Physician to the North Infirmary, and to the Cork Workhouse. The following cases of poisoning by carbonic acid gas seem to present some features of interest:—

Five sailors, Danes and Swedes, were admitted into the Cork North Infirmary on February 10th, 1860, from the brig Helgesen, lying at Merchant's-quay. It appeared that the vessel was infested with rats, which the captain determined to destroy by charcoal vapour; accordingly, all the crevices of the hold were carefully closed, in order to confine the gas as much as possible to the lower parts of the vessel. The sailors, five in number, were obliged to occupy an ante-room to the captain's cabin, recently painted; three of them slept in the room itself, and two in a closet off the room,—the dimensions of the former being four feet by seven—those of the closet, barely sufficient to hold a bed,—the doors and windows of both being made air-tight for the time. At about 5 o'clock in the morning, the watchman heard moans proceeding from the ante-room; and on opening the door, he found the men in a state of complete stupor. On going into the captain's sleeping-room, which was better ventilated, he also was discovered insensible, but still capable of answering when roused. Medical assistance was sent for, and Dr. Wycherly and myself attended without delay. We had the men wrapped up in warm blankets, and brought on deck, all of them, except the captain, incapable of being wakened up from stupor; their mouths were covered with white foam; the lips livid; the teeth so spasmodically

closed, that it required a strong lever to force open the jaws, which shut with a snap, when the plug was withdrawn. Their faces were deadly pale; the eyelids closed, offering much resistance to every attempt to raise them, the pupils being insensible to light, and much dilated. Their breathing was very laborious and with stertor; the number of respirations about 8 or 10 in the minute. The pulse averaged about 50, and was feeble and flickering, not felt in the smaller arteries. The temperature of the surface of their bodies was very low, both the hands and feet feeling like marble. When the men were brought into the air, after about half an hour, with the help of remedial measures, they were so far revived from their deep lethargy as to show a slight return of consciousness when they were strongly roused.

We were limited in our remedies, in consequence of the early hour at which the accident occurred, and the want of medicines in the ship; but manifest benefit was gained in each case by assiduously rubbing the surface of the body, keeping hot bottles to the stomach and feet, stimulating the nervous system by means of sinapisms along the spine, and when the power of swallowing began to return, giving mustard emetics, which we found of use, both unloading the stomach and exciting the whole system. I need hardly say that great difficulty was felt in administering these remedies, as the teeth had to be forced asunder,—the men resisting our efforts, and showing an obvious dislike to be stirred, and a desire to be allowed to sleep on. After a couple of hours, consciousness was a little more restored; and as the ship was a place quite unfit for them, they were removed to the North Infirmary, and placed in a well-ventilated ward. At this time their state was as follows: the feet intensely cold, foreheads hot; the muscles supporting their heads quite relaxed, so that they had to be kept up by pillows; their faces either livid or purplish; but the bloated and distorted condition of the features at first evident had settled down into the calm of profound sleep, so that as they were ranged side by side, all similary locked in deep slumber, the imagination could scarcely avoid conceiving them to be bound by some magic spell. In addition to the remedies employed, cold affusion was applied to their heads, and aromatic spirits of ammonia and camphor were freely administered, with hot coffee as strong as could be made. In a few hours reaction took place, the circulation rose; and flushing of the face, with strong throbbing of the carotid and temporal arteries, came on, which symptoms were kept down by antiphlogistic means. When they recovered consciousness, they complained chiefly of vertigo, and frontal weight and pain as if the head would burst, also of noises in the ears, weak sight, and other symptoms of perverted nervous function, loss of speech continuing some time, and succeeded by indistinctness of articulation, and obvious confusion of ideas. The secretion of urine was totally arrested in all the cases for a considerable time. It took from 36 to 48 hours to restore them to *full* consciousness, most of which time was passed by them in sleep, some of

the patients were longer than others in regaining it. On the third day the drowsy stupor had passed off, and on the fourth they were discharged well.

Remarks.—In the preceding cases we noticed the effects of poisoning by carbonic acid gas from two co-operating causes, viz., the non-renewal of the air in their sleeping apartment, and the direct inhalation of the charcoal vapour, which, notwithstanding its high specific gravity, must have pervaded the ship; as the captain, who slept in a separate cabin, did not escape its effects. Though the five sailors were exposed during the same period of time, and to the same amount of poison, the power of resistance varied in the respective cases, some being more heavily stupefied than others. The boy appeared more powerfully affected than the men, but recovered more speedily.

The effects of carbonic acid gas upon the system are more deadly than those which arise from the simple deprivation of oxygen, they are, as Copland has well shown, not those of mere asphyxia; but of actual poisoning. If we breathe an atmosphere of pure nitrogen or hydrogen, death ensues from a passive cause, namely, the want of the elements of respiration, whereas carbonic acid is an active agent. Animals immersed in pure carbonic acid die in half a minute, whereas in the other two gases they live much longer. Besides, carbonic acid when pure, causes spasm of the glottis, thus rendering inspiration difficult, if not impossible.

As to the psychological effects produced in the above cases, they were, as far as could be ascertained, much more of a painful than a pleasurable kind. It is stated that the sensations felt during drowning are not so distressing as our great dramatist represents them to be in Clarence's dream,—

> "O Lord! methought what pain it was to drown,
> What dreadful noise of water in mine ears," &c.;

but it is, at least, certain that the sensations experienced during returning consciousness are eminently painful. Those felt in cases of poisoning by carbonic acid are still more distressing, as being more prolonged. Body and soul appear to be weighed down by an insupportable incubus; the phantoms which flit before the imagination produce awe and terror: such were the sensations in the above cases, as far as they could be recalled; but they were so poignant, that the bare recollection of them was a subject on which the men avoided to dwell.

Cases of Facial Paralysis. Read by Professor O'Connor.—Paralysis of the portio dura of the ninth pair of nerves is of such frequent occurrence, and generally of such easy diagnosis, that it affords more interest to the physiologist than to the practical physician. Still cases are occasionally met with, in which the unguarded physician may be led into serious error, in not duly recognising the cause of the disease, and consequently underrating its danger

—an error the more likely to occur, from its generally mild character. The cases I am about to relate are many of them of frequent occurrence. Others of them are such as may be only seen in a practice extending over very many years.

The causes of this affection are very diversified; still they may be reduced to three heads,—symptomatic, organic, and mechanical. Under each of these heads, I will relate a few of the cases which have come under my notice. A lady, in the last months of pregnancy, had very confined bowels. They had not been affected for nearly a fortnight. In such cases we are prepared for great disturbance of the nervous system. She complained much of headach, vertigo, defect of vision. These symptoms she did not attend to; but on sitting to breakfast one morning, her friends were alarmed at seeing her mouth drawn to one side, when she attempted to laugh. A few doses of active purgative medicine removed the whole of these alarming symptoms, showing clearly that in this case the paralysis was purely symptomatic.

A child about fifteen months old was sent to me a short time since. The nurse stated that whenever the child cried, his left eye was forcibly closed, the right remaining in an open stare; and that the same eye remained partially open in sleep. It is not difficult to make a child cry in a physician's study, so I had soon an opportunity of witnessing the state of things referred to by the nurse. I observed that not only did the right eye remain open during crying, but that the mouth was drawn to the left side. The nurse stated that the same occurred whenever the child laughed. It was not difficult to recognise the disease as facial paralysis. Still I was not freed from serious alarm, fearing that the origin of the disease might exist in the brain, and be the precursor of hydrocephalus, of which an infant, belonging to the same parents, died a short time previously. In looking over the records of cases of this disease, I could not find one at this early age. After a most careful watching of the child for some days, I became satisfied that no other part of the nervous system was affected. At one of my visits the nurse announced a great improvement in the symptoms, which coincided in time with the appearance of a jaw-tooth. From this time forward the deformity became every day less, till it finally disappeared. In this singular case, the disease was clearly produced by difficult dentition. Under the head of organic causes, may be mentioned those cases which result from impressions on the extremities of the branches of this nerve by draughts of cold air, eruptive diseases, or by rheumatic inflammation of the neurilemma; although, in these cases, structural change in the nerve is more a matter of inference, than capable of demonstration.

A young female was under my care, suffering from an herpetic eruption on the right cheek. This receded suddenly, and was immediately followed by paralysis of the portio dura of the sound side, which was not removed for several months. It is more common to see the sensitive powers affected by suppression of her-

pes than a nerve of motion. However, both are met with frequently enough. A gentleman, who got out of his bed at night, was suddenly exposed to a draught of cold air from the window, and became seized next day with this affection. A patient, now under my care in hospital, became similarly affected from sleeping in the open air. In all these cases, we must believe that some structural change took place in the nerve. The third, or mechanical cause, is the more frequent, and is more or less free from or attended with danger, according to the situation of the pressure, as to whether it occurs within the cranium, in its passage through the bone, or external to it. Cases of the latter are seen after using the forceps in the delivery of a child. The deformity in these cases is sometimes transitory, sometimes permanent, in proportion to the amount of injury done to the nerve. A coachman received a blow of a sweeping-brush (accidentally) behind the ears. He was for some time insensible, and on his recovery was found to be affected with the ordinary symptoms of facial paralysis, without any symptom of cerebral disease. The disease in this case continued during life.

A case came under my notice in the workhouse, in which a considerable portion of the temporal bone was denuded by cancer. In this there was paralysis of the muscles supplied by the portio dura. A young female had caries of the petrous portion of the temporal bone. Abscesses formed, which burst externally, and dried up after some time, leaving, however, permanent deformity from injury to the portio dura in its passage through the bone. It is when the origin of the disease exists within the cranium, either from morbid growths, or the products of inflammation pressing on the nerve at its origin, that the greatest danger exists.

In these cases there is reason to apprehend that a further extension of the diseased structure may press on the brain, and produce fatal results. Some time since, in consultation with a member of this society, I saw a gentleman whose principal symptom was facial paralysis. He had, however, in addition, great pain in the affected side of the head, extending to the occiput, and some uneasy feeling on turning his head, which he avoided as much as possible. He was also somewhat deaf on the side at which the paralysis existed. We formed the opinion that the origin of the paralysis was within the cranium—perhaps disease of internal table of the occipital bone. He died after some time of cerebral disease, which was preceded by symptoms resembling caries of the odontoid process.

I had an opportunity of witnessing at the workhouse a rare form of this affection, namely, paralysis of the portio dura of both sides. The patient had been suffering for some time from secondary syphilis, especially from periostitis of the cranial bones. Paralysis of the portio dura occurred first on the left side, and after a little time the opposite side became affected. Still the hearing was not injured, nor the sensorium disturbed,—though to look at his countenance, he seemed to be perfectly idiotic. His features had lost all expression. The eyes were in a constant stare, bloodshot, and suf-

fused with tears, which also trickled down the cheeks. The angles of the lips falling down permitted the saliva to flow over, as also some portions of liquid, whenever he attempted to swallow, which was performed with difficulty. As he could not use the lips in speaking, his voice was guttural, coming as if from the back of his throat. As the disease was not supposed to be dangerous to life, his countenance produced more a feeling of ridicule than of pity among his fellow patients, which compelled him to leave the hospital before I could learn the termination of the disease.

I hope the recital of these cases may justify me in bringing under the notice of the society this trite subject, as it will be perceived that the disease is not always easy of diagnosis, nor trivial in its results.

Hypertrophy of Heart; Disease of the Aortic Valves; Pericardial Effusion; Duodenitis; Jaundice.—Dr. Finn communicated the particulars of this case, exhibiting at the same time the pathological specimens.

John Kelcher, aged 17, was admitted *in extremis* into the North Infirmary, on the 4th of November, 1860. He had been labouring under cardiac disease for more than a year; and the difficulty of breathing, from which he occasionally suffered during the earlier period of his illness, had latterly become so urgent as to preclude horizontal decumbency. He has also been much troubled with cough; but never had rheumatism, nor has he inherited any tendency to this disease. His employment, that of assistant in a tobacco manufactory, and excessive indulgence in the use of tobacco (which he both smoked and chewed), had probably contributed to aggravate, in no ordinary degree, the cardiac symptoms. Soon after admission to hospital, he complained of irritability of stomach, which was the precursor of an attack of jaundice. *General Symptoms:*—Loss of appetite, coated tongue, full, rapid, and bounding pulse; and œdema of lower extremities. *Physical signs:*—Visible pulsation of the arteries generally; cardiac fremitus; loud bellows' murmur, replacing both sounds of the heart. He died on the 16th of November, having suffered for some days previously, both from epistaxis and hemoptysis.

Autopsy (*Chest*).—The lungs were universally adherent; there existed considerable congestion in their inferior and dependent parts. The pericardium contained a large quantity of serous effusion. The heart, which presented a globular form, was greatly hypertrophied; the thickness of the parietes of left ventricle, at the base, centre, and apex, was nearly threefold greater than that of the normal standard. The mitral valves were much swollen, but in other respects healthy; the aortic valves were corrugated, and studded with deposit to such an extent, as utterly to preclude the performance of their office. The portion of the mucous membrane of the duodenum, contiguous to the opening of the ductus choledochus, was

deeply congested and thickened. The gall-bladder contained a large quantity of inspissated bile, and some biliary calculi.

The President read the following communication, which he had received from his friend Dr. Carey, of the Royal Artillery, stationed at Ballincollig:—

Sir,—My attention having been lately directed to the fact of ague existing to some extent in this neighbourhood, I have the honour to lay before you the result of some inquiries I have made relative to that disease. The subject is an interesting one, not only from the knowledge of ague existing to the extent that the numbers furnished exhibit (when we consider that Ireland is not held to be an aguish country), but also from the importance that such a circumstance has in connexion with the large body of troops constantly stationed in and about this neighbourhood. That ague does exist, to a very great extent, I am fully satisfied. The statistics furnished were supplied to me by an intelligent and practical physician, who has had long experience in this country, and ample opportunity of observing the disease, with its various exciting and predisposing causes. To me this is a peculiarly interesting topic, as, during my residence here, the troops under my charge have enjoyed a strange immunity from ague, while around them on every side the disease has made its havoc.

Before I enter on the subject of the disease itself, I deem it necessary to furnish you with as much of the topography of this district as I can, such being the key to the right understanding of the nature and habits (if the term can be applied) of this disease.

From the city of Cork to about twenty miles west of Ballincollig, the country is divided into two districts by the River Lee. To the north it is hilly, the soil fertile, with a substratum of *brown sandstone*. There is a good deal of marsh between small hills. There is a large lake in the neighbourhood of Blarney, about four miles from this place; and the country about Blarney is marshy, and intersected by many running streams. In winter, several acres are entirely covered with water. One hill, about five miles to the N.W. of this barrack, is higher than all the rest, and on this hill more ague has occurred than in any other part of the country. There is a good deal of wood at this side of the River Lee. To the *south* of the river, the country is much more flat for some distance, very marshy; and this marsh extends, in a long vein, for several miles to the west. To the left of this marshy vein, there is a range of low hills, and from these numerous rivulets flow into the flat district under them. The soil here is in parts fertile, but the substratum of stone is *limestone*. This is of two kinds—one hard, mixed with black marble, the other *dolomite*, very acrid and burning when in a state of mud. To the *west*, the country is wild, and badly cultivated; and about one mile from the barrack there is a pond on the road-side, which I am informed is a fertile source of ague in the spring; at least, the disease in that neighbourhood increases consi-

derably when the water subsides in this pond. To the *east*, and towards Cork, there is a long flat district, a continuation of the marshy vein I mentioned before; and included in this vein is the western suburb of Cork, a spot bearing the strongest features of an aguish district, and in which I saw a very bad case this summer. It occurred in a house situated on the edge of what is called "the Dyke"—a stream running between a marshy field leading down to the River Lee, and a gravelled promenade running parallel to the public road, which is about fifty yards distant.

This barrack is situated about five miles to the west of Cork, about midway between the range of hills to the north and the flat vein of marsh to the south I before mentioned. Of its internal structure you are already fully aware; and although its deficiencies, in some sanitary respects, require amendment, still you will agree with me in thinking it a strange fact, that within a period of a year I have had but one case of ague, and that occurred in a constitution peculiarly predisposed.

In connexion with the barrack is a large powder-mill, extending over several acres. The various small mills constituting it are supplied with water by canals, running in every direction. There is a good-sized village in the enclosure, inhabited by the families connected with the working of the mills. The village is within four hundred yards of the outer wall of the barrack, and among the inhabitants of this village ague runs its course to a very great extent. There is a point which I deem of much importance to notice, when speaking of the marshy nature of some of the country about here; it is commonly called *boggy*, but it is not a peat country. There is no turf or peat cut in the country for many miles round; and it will be shown by a comparison with other parts of Ireland, where there is quite as much wet ground, that, as far as we know of the disease ague, this circumstance is of much interest.

The *form of ague* principally seen here is the tertian; some cases of quotidian have been seen, also several cases of remittent fever among children, and some of these while at the breast. They are all of a severe type, but have been generally treated with success. They occur at all months of the year, but principally in March, April, and May. I have been told that when easterly winds prevail, ague is worst, and most frequent; but this year the prevailing wind has been N. W., and this has been the worst year for a long time. Ague has been increasing of late years. The Table annexed will clearly point out these circumstances.

It is not for me here to attempt any general disquisition on the real nature of the poison that gives rise to this distressing disease in many quarters of the world; it has been met with, ably discussed, and now almost entirely under the control of a few specifics. The influence of wet soils, decaying vegetable matter, certain prevailing winds, &c., are well known; but there is one point that, perhaps, is not so much thought of. Many years ago, while I was a student, I heard an able physician in Dublin (Professor Osborne)

state that he thought the absence or presence of a peaty soil had much to do with the prevalence or otherwise of ague in Ireland; and my own experience, and the account of even unsophisticated people, have strongly impressed me with the value of the assertion.

For nearly twenty-two years I resided constantly in the vicinity of a large peat bog—one that supplied turf to many parts of the Upper and Lower Shannon. While walking through many marshy districts near this bog, the peculiar dark-coloured water exuded proved the presence of peat; and it was so strongly impregnated with the colouring matter contained in it, as to leave a permanent stain in clothes, and even in the skin of the feet, which could not be got rid of until the cuticle came off. I have frequently remarked the astringent taste of this bog-water—of its preservative and antiseptic quality there is no doubt. I have seen animal substances, such as butter (which has been buried in a bog for two years), taken up perfectly sound, and have been told of bodies exhumed after a long period almost unchanged in condition.

It has been suggested by a well-known author, that the existence of "tannin" in peat-bog was, perhaps, the cause of the absence of ague in its neighbourhood; but whatever the cause may be, I have not seen, for the twenty-two years I lived near one, a single case of idiopathic ague, and for ten years of that period I was constantly engaged in assisting in a large dispensary district.

A strange fact was mentioned to me a short time ago by a non-professional gentleman. In a district on the Upper Shannon, one side of the country is a peat country. The River Shannon flows between this and the other side, which is marshy, but has no peat; and the marshy side is, as he said, "full of ague," while the other side is said not to have any. That ague selects for its victims those who are debilitated, and whose nervous systems are unusually depressed, is, I believe, generally received. There can be little doubt that the poor of this district are readily susceptible of it—badly clad, badly housed, and badly fed. The mud hut and clayey floor exhaling constant damps, and above all the filthy cess-pool at the door,—the plague spot of an Irish hovel,—must lay them open to a disease whose favourite nidus is found in damp places devoid of a circulation of air.

An officer, who had been a good deal in Canada, told me he could distinctly recognize the kind of atmosphere where ague existed, and could predict the occurrence of ague by a peculiar smell in the atmosphere, known best to those who have once perceived it. A gentleman in this neighbourhood got concussion of the brain, not long ago; and when debilitated by the attack, and consequent confinement to the house, had a sharp attack of ague, to crown all; and in my own practice, a patient was liable to the disease for a period of two years, who had been subjected to repeated courses of that depressing drug, mercury, and did not lose the disposition to it until he went to a hot and dry climate.

I may again recur to the infrequency of ague in this barrack;

and may in all fairness ascribe it to the difference in the circumstances and condition of the troops, contrasted with those around them; the good food, good clothing, regular habits, and exercise of the men must be the best prophylactic to a disease on the watch for the reverse; and though occasionally, in other climates, soldiers do fall victims, the causes might be found in some depressing circumstances, such as a long march previous to encamping in an agueish district, and that too at night; or a deficiency in the supply of proper food, or wisely-administered stimulant.

Year Commencing January, 1854.

1854.	Ague.
January,	None.
February,	10
March,	16
April,	13
May,	9
Total,	48

1855.	Ague.
January,	None.
April,	20
May,	7
Total,	27

1856.	Ague.
April,	18
May,	22
June,	7
July,	21
August,	4
September,	3
Total,	75

1857.	Ague.
February,	3
March,	14
April,	13
May,	27
June,	16
July,	13
August,	5
September,	4
Total,	95

1858.	Ague.
February,	6
March,	21
April,	40
May,	36
June,	21
July,	14
August,	8
September,	2
October,	4
Total,	152

1859.	Ague.
January,	1
February,	7
March,	8
April,	28
May,	38
June,	22
July,	3
August,	5
October.	1
December,	1
Total,	114

1860.	Agne.
January,	6
February,	2
March,	15
April,	41
May,	51
June,	15
July,	8
August,	3
September,	11
Total,	152
Grand total,	663

JANUARY 9, 1861.

DR. WALL in the Chair.

DR. W. C. TOWNSEND brought forward the following case:—

James O'Neil, aged 25, was admitted into South Charitable Infirmary, December 6, 1860. Patient, a labourer, states that three years ago, when mowing, he spit a small quantity of blood, and that he has done so since frequently. His health had been good previously; but since these attacks came on, he has found himself getting thinner and weaker, and has been much troubled with cough. He was admitted into hospital labouring under a severe attack of hemoptysis. The symptoms were, frequent cough; expectoration bright red, nearly all blood, only a small quantity of mucus. Right side of chest expands better on inspiration than the left; more extensive dulness on percussion at the apex of the left lung than at that of the right. Bowels confined; appetite bad; decubitus easy in any position. Ordered four ounces of Epsom salts, half an ounce of dilute sulphuric acid, sixteen ounces of distilled water. Make a mixture. One wineglass full to be taken every three hours till it operates. Five grains of mercury with chalk to be taken at bedtime.

December 7th.—Unable to sleep last night, from the frequent cough. Spit three cups-full of blood; complains of slight headache. Continue medicine.

9th.—Hemoptysis checked; expectoration only slightly tinged; bowels free; no head-ache; pale countenance; pulse 102; slept most part of the night. Omit the acid saline mixture. Take of dilute sulphuric acid, two drachms; simple syrup, one ounce; water, eight ounces. Mix. Let him have one ounce every three hours. Repeat the grey powder every night.

11th.—Cough troublesome; no appearance of blood in sputa, which is very purulent and copious; bowels regular; free from pain anywhere; pulse 102.

12th.—Seems rather stupid this morning, and comprehends what is said to him with difficulty; feels his back stiff, and is unable to lean forward. Same in other respects. Omit the sulphuric acid mixture. Let him have one ounce of citrate of potash mixture every three hours. Repeat the grey powder at night.

13th.—Very heavy and stupid this morning; has a strong typhoid smell; tongue brown, and loaded; passed urine and fæces involuntarily in bed; bowels very loose; complains of no pain; decubitus on either side. Ordered an enema of starch, with two scruples of laudnum. Half a pint of port wine at intervals during the day. Evening. Very weak, and comatose; taking a considerable time to take in his tongue after protruding it; hard to be roused to consciousness, immediately relapsing into stupor. Ordered a blister to the back of neck, blister over the region of heart if necessary; hot stupes to the feet; eight ounces of port wine.

14th.—Tongue very loaded and brown; stupor increased; roused with more difficulty, relapsing immediately into a comatose state. Discharged his stomach frequently, and does so after taking any drink; bowels free; passes urine and fæces in bed; very heavy typhoid smell. Was moved into a private ward. Continue two ounces of wine every third hour, and one ounce each of brandy and soda-water every second hour. Evening. A catheter was passed, and ten ounces of urine drawn off. Bowels moved twice in bed; stupor increased.

15th.—Breathing stertorous, and all the other symptoms increased; bowels act unconsciously; kept up with brandy; the stertor increases, and a very heavy typhoid smell. Sank rapidly since 9 A.M., and expired at 2 A.M. next morning.

Autopsy. Twenty-one hours after death.—All the vessels of the brain much congested; slight serous effusion at base; falx cerebri inflamed, and adherent to the brain. The substance of the brain was studded with small abscesses, and softened tubercles. The ventricle was healthy, and so was the cerebellum.

This man was brought into the South Infirmary, suffering from a severe attack of hemoptysis, consequent on advanced tubercular disease. Owing to the arrival of his friends, it was impossible to examine his chest or abdomen.

The question to be determined was, the cause of death. Did the patient die of pyœmia; or could he in any way have contracted the poison of typhus?

Cases of Gastric Ulcer. By DR. CUMMINS.—Drs. Brinton and Budd, in their valuable works on diseases of the stomach, have dwelt much on the danger of roughly handling the epigastrium, when there is reason to suspect disease of the stomach; and I lay the following short communication before the society, principally with a view to illustrating this point.

On the 1st of August last, I was called to see an elderly lady, who had for many years been the subject of gastric disease, and had been under the care of some of the first physicians of the day at various times. She had occasionally vomited small quantities of blood; and had more or less, throughout the entire period of her illness, suffered from a gnawing pain at the pit of the stomach, and corresponding part of back, increased by eating meat, and various other articles of food, as well as by mental emotion. The bowels have always been costive, requiring the daily use of a lavement; but in other respects the general health has been good, and her habits have been remarkably active. There is nothing in the appearance to create suspicion of malignant disease; and as vomiting has never been a symptom of her complaint, I concluded that the orifices of the stomach were not involved in any disease that might be present. She has also been at all times free from acute, or darting pain.

The fortnight previous to my first visit had been one of more suffering than usual. She was still, however, in the intervals of pain, able to go about, and join the family circle at meal-times.

On careful examination of the epigastrium, I discovered a tender spot, about its centre, corresponding to the situation of a small tumour, which, however, was not very distinctly perceptible.

When I stated to her my belief that she was the subject of ulceration of the stomach, she did not disguise her incredulity; and, to prove that I was mistaken, commenced thumping the part with her hands, and continued doing so for some minutes, notwithstanding my most urgent remonstrances. The consequence was, that almost immediately after, she suffered from a most violent paroxysm of pain, which subsided in about ten minutes, leaving her much in the same state as before. I prescribed a mixture containing bismuth and morphia, which, however, was not taken, as she felt better in the evening, and early part of next morning.

I fortunately stated to her friends my apprehensions of a fatal termination at some time, from either perforation or hemorrhage; but I was not at all prepared for the rapidity with which my words were to be verified. Early next morning, I received an urgent message; and as the distance was considerable, did not arrive for nearly two hours after, when I was informed that she had risen early, in her usual health and spirits, with the intention of going to town; that she had partaken of an excellent breakfast, and gone to her room to dress immediately after, when her maid, hearing her bell ring violently, came in, and found her suffering from agonizing pain in the stomach, accompanied by vomiting and faintness. At one glance I saw what had happened. There was no mistaking the collapsed expression, the knees drawn up, and hands extended to prevent any one touching her, the tympanitic distention of abdomen, the rapid thready pulse of 160, and all the other symptoms of peritonitis from perforation. I immediately sent for Dr. Meade, and, in concert with him, prescribed a grain of opium every hour, which had the effect of mitigating her sufferings, and prolonging her life until past midnight, when she sank gradually.

It was very unfortunate that no post-mortem could be obtained, not that there could be any doubt as to the cause of death, but to find out whether perforation had taken place at the time she so violently thumped the epigastrium; and if it did so, by what mechanical contrivance nature prevented effusion into the peritoneum, until after a full meal had distended the stomach.

I think every one who has heard my description of the history of the case, written at the time of the occurrence, will believe with me that the rough handling of the epigastrium was connected with the perforation, whether the latter occurred at the moment or not. We have all seen or read of the wonderful mechanical contrivances by which effusion of blood is sometimes temporarily stayed, after the bursting of aneurisms; and we can easily believe that something of the kind postponed the fatal effusion here.

It is probable that an ulcer, or ulcers, had been in existence in her stomach, alternating perhaps with periods of cicatrization, for many years; and that during the fortnight previous to my visit, which, as I already stated, was marked by great increase of pain, the ulceration had extended to the peritoneal coat. It is possible, also, that cancerous matter might have been latterly deposited in the "hard brawny mass" that generally surrounds those ulcers, and given rise to the tumour indistinctly felt; or possibly it might have been caused by adhesion of the stomach to some neighbouring viscus.

The diagnosis of ulcer of the stomach is at all times involved in some obscurity; but when long-continued gastric disease is present, accompanied by hematemesis, and the peculiar gnawing pain of epigastrium and back, increased immediately after taking food, all of which were met with in this case, we have a group of symptoms, quite sufficient for a decided diagnosis; but when these were followed by peritonitis from perforation, there could be no doubt whatever as to the lesion that had existed during life. Had this lady lived a little longer, I should have put her on a systematic course of diet, such as Dr. Brinton recommends, with some hope of amelioration, if not of recovery, by cicatrization. I have seen great benefit result from such a course of treatment in similar cases; but unfortunately there is a great tendency to relapse afterwards, from the least imprudence in diet.

Anal Orifice within the Labia Majora, separated from the Vagina by a Mucous Septum. By H. P. Browne, M. D., K. & Q. C. P., and L. R. C. S. I.; Physician to Delgany Dispensary.

On the 18th December, 1860, I was requested to visit A. E., aged 35, the wife of a farmer residing in this district. On my arrival I found the woman collapsed, and pulseless, from excessive hemorrhage. Having ordered stimulants, and restored heat, I learned from her that she feared it would be a premature confinement. I then made an examination *per vaginam;* and finding an abnormal condition of parts, I requested the midwife in attendance to raise the bed-clothes, in order to satisfy myself by an ocular examination. To my great surprise, I found the termination of the rectum situated within the labia, and in close approximation to the vagina, separated from it merely by a mucous septum; so that you could pass the fore and middle fingers into the two openings at the same time.

I found the hemorrhage in this case controlled temporarily by the exhibition of the tincture of larch bark, as recommended in other cases by Dr. Frizelle.

On the 20th, strong abdominal pains set in, and the woman aborted, having expelled a fœtus of four months.

This patient is the mother of four healthy children. She states her labours have been always very tedious, doubtless arising from obstruction caused by the abnormal position of the rectum.

On the Preparation of Chloride of Zinc in Cylinders. By M. Sommié.

The cylindrical form for certain topical applications is coming into more general use. We have the white and black nitrates of silver, caustic potash, and sulphate of copper. M. Sommié recommends the preparation of cylinders of chloride of zinc by the following method:—

Soften gutta percha in boiling alcohol, and incorporate it in a heated porcelain mortar with an equal weight of chloride of zinc in a state of fine division. Roll the mass rapidly on a marble slab, after the manner adopted in the manufacture of pastiles. The cylinder having been reduced to the diameter of a quill, is divided into several lengths, and each fragment is tapered at its extremities. The pencils must be kept in wide-mouthed bottles, surrounded with powdered lime.

These pencils remain solid, of uniform causticity, and are easily manipulated. They act like a sponge, slowly exuding chloride of zinc, and liquify when in contact with the air or the skin.

For Canquoin's paste or chloride of zinc in slabs, gluten is preferable to flour as an excipient; it is more elastic and less hygrometric.—*Bulletin Général de Thérapeutique*, 15 June, 1860, p. 499.